BURNETT LECTURES.

ON LIGHT.

NATURE SERIES.

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ON LIGHT.

In Three Courses.

DELIVERED AT ABERDEEN IN NOVEMBER, 1883, DECEMBER, 1884, AND NOVEMBER, 1885.

BY

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PREFACE.

THESE Lectures originated in a new direction given to an old endowment. Mr John Burnett, merchant in Aberdeen, who died in 1784, bequeathed the greater part of his property to various charitable and pious objects. Among others, a portion of the property was vested in trustees for establishing prizes for the best and next best essay on the following subject:—

"That there is a Being, all-powerful, wise, and good, by whom everything exists; and particularly to obviate difficulties regarding the wisdom and goodness of the Deity; and this, in the first place, from considerations independent of written revelation,

and in the second place, from the revelation of the Lord Jesus; and, from the whole, to point out the inferences most necessary for, and useful to mankind."

These essays were to be competed for at intervals of 40 years, and awards have been made on two occasions since the original foundation.

But it was thought that the production of essays at such long intervals did not form a satisfactory mode of utilizing the bequest of the founder; and in 1881 a new direction was given to the foundation by an order of the Secretary of State for the Home Department, in accordance with the provisions of the Endowed Institutions (Scotland) Act of 1878. By this order it was provided that a Lecturer should be appointed at intervals of 5 years, to hold office for 3 years, the subject of the lectures being determined by the following regulation:

"The Trustees and assessors may prescribe as the subject of the course of lectures that specified in the codicil to the will of the testator, viz. [as above]. Or

otherwise, the trustees may prescribe, as the subject of any course of lectures, recent researches (as at the date of the appointment) into any of the following branches of knowledge, viz.:—

1. History, including the illustrations of the forms and effects of theistic doctrines among the older nations of the world.

2. Archaeology.

3. Physical science.

4. Natural science.

And they shall instruct the lecturer to have regard, in treating of the special subject prescribed, to the illustration afforded by it of the theme proposed by the testator, and that under such conditions or qualifications as they may prescribe."

The Trustees selected physical science as the subject of the first lectures under the new system, and they did me the honour of appointing me the first Burnett Lecturer. It was arranged that four lectures should be given each year of office, and that the subject of the complete course should be Light, this subject being treated under three divisions, to which the lectures in each year should be respectively

devoted. The first year's course was to be on light considered in itself: in other words, on the nature of light. In the next year light was to be considered as a means of investigation, which would give occasion for reference to recent researches connected with this subject. The third year's course was to be devoted to a consideration of the beneficial effects of light, a subject which would naturally harmonise with the original intentions of the founder of the trust.

In the first course it was my aim to give the audience some fair idea of the evidence on which we accept the views respecting the nature of light which are at present held, I may say universally, in the scientific world; such an idea as it might be possible to convey in lectures which were necessarily of a rather popular character, and in which the use of mathematical reasoning had to be almost entirely avoided. In the second course I endeavoured to explain the mode of arriving at certain

highly remarkable conclusions which have been reached through researches carried on in years more or less recent. The benefits derived from light, which form the subject of the third course, are, it might have been supposed, too obvious to require mention. Yet few perhaps have been in the habit of contemplating these benefits as a whole, or have perceived how far-reaching and of what vital importance are the advantages that we derive from light, if we include in that term, not merely what the eye can perceive, but all that in its physical nature differs from visible light only in the way in which light of one colour differs from that of another colour.

G. G. STOKES.

CAMBRIDGE,
May, 1887.

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p. 296, l. 17. *For* "thousandth", *read* "ten-thousandth".

LECTURES ON LIGHT.

FIRST COURSE.

ON THE NATURE OF LIGHT.

LECTURE I.

Bearing of the more obvious properties of Light on our view of its nature—Theories of emission and of undulations—Colours of thin plates—Newton's attempt to explain them on the theory of emission—Insufficiency of the explanation.

AMONG all our senses, there is none more wonderful than that of sight. It confers upon us, as Sir John Herschel has remarked, to a considerable extent the character of ubiquity. It is accordingly a matter of extreme interest to find out what we can as to the mode in which this end so important to our well-being is brought about; whether it be by investigating the properties of any agent external to ourselves which may be concerned in its accomplishment, or by seeking to penetrate some little way into that

mysterious chain of sequence which connects the external agent with the sensation conveyed to our minds. It is true indeed that there is no prospect of our being able to bridge over the gulf which separates mind from matter; yet there are many things to indicate that that mysterious organ which we possess, the brain, has some intimate connexion with the operations of the mind, and we can do something towards tracing a connexion between the part of our bodies directly affected by the external agent and the brain; a connexion of such a kind as to leave no doubt that it forms the means whereby the immediate action of the external agent is ultimately perceived by the mind; though how the final conveyance takes place is a mystery we are not likely to fathom. But this does not prevent us from being able to trace some links in the chain of connexion, nor from recognizing the evidences of design which that portion which we can in a measure follow is calculated to impress upon us.

I have spoken of an external agent even though it was more especially the sense of sight that I had in view. We are so accustomed to the contemplation of an objective *something*, which we call Light, as the external agent by the action of which vision is in some way brought about, that we have a difficulty in conceiving how anyone could think otherwise. We

see the objects in a room, but it is matter of the commonest observation that though the objects may be there and our eyes may be there, we see them not if it be night, or if the room be closely shut up, until a lighted lamp or candle or something of the kind is introduced. We recognize the flame as the seat of some influence, to which we give the name of light, which is essential to vision. Yet obvious as this proposition appears to us, it is remarkable that it was not always so. At least one writer of acute intellect in ancient times held that it was something emanating from the eye, not something from without entering into it, that enabled us to perceive distant objects. This shows by what a slow and gradual progress our knowledge of physical science is built up. We are accustomed to vaunt of our knowledge in this 19th century; yet it may be that generations hence the scientific men of the day will wonder how we could have failed to perceive things which to them will appear quite obvious.

A self-luminous body then is, as such, a source of an influence which can be exerted at a distance. In this respect it by no means stands alone; two bodies at a distance from one another may for example influence each other through the attraction of gravitation, and other modes of influence might also be mentioned, but I will confine myself to gravitation.

Now between these two modes of influence there are great and striking differences. One of the most salient is this, that in the case of gravitation the influence is exerted independently of the interposition of matter of any kind, whereas in the case of light the influence is capable of being arrested by an extremely thin screen of matter of a suitable kind; for example, silver foil, or a film of Indian ink spread on glass.

The consideration of a screen leads us naturally to another fundamental and very obvious property of light. Suppose the screen pierced by one or more rather small apertures; how will the influence beyond the screen be distributed? We find that it is perceived only within the projections of the aperture or apertures made by straight lines drawn from the luminous body, which for simplicity I here suppose to be a point.

This indeed is not rigorously true, for about the boundary of the projection of the aperture, that is, near the projection of the edge of the aperture, there is a gradual, not sudden, passage from light within to darkness outside, accompanied by fluctuations of greater and less luminosity which I cannot now enter into particularly; and when the aperture is extremely small, the spreading out of the light which passes through it is by no means very small compared with

the breadth of the projection of the aperture itself. Nevertheless in ordinary cases the spreading out of the light is so small that we may disregard it, and say that light proceeds from a luminous point in all directions in straight lines until it is stopped by some obstacle. It will be understood that I am here speaking of light only as it passes in free space, or which comes to nearly the same thing, in air, and not from one medium into another.

When I speak of light proceeding *from* a luminous point, all I wish to express is that we recognize the luminous point as the seat or origin of a certain influence which is exerted, though with an intensity which diminishes with increasing distance, at all points from which a straight line can be drawn without interruption to the luminous point; I do not wish to imply the idea of motion, or propagation of any kind. The feature therefore that we are at present considering is common to light and gravitation.

A very important question now arises, Do these influences take time to travel, or does the influence exerted at any moment of time depend solely on the state and position of the two bodies, the influencing and the influenced, at that moment? The idea which we may be led to form as to the nature of light must depend most materially on the answer we have to give to this question as applied to light.

That light is propagated in time, cannot be inferred from ordinary observation. Thus when a landscape is illuminated at night by a flash of lightning or an explosion of gunpowder, the flash and the objects illuminated by it are seen, so far as our senses can decide, simultaneously, though in the latter case the light has to travel from the flash to the object illuminated, and from that to the eye, instead of coming straight from the flash. As far as ordinary observation goes, then, the question whether any time is occupied in the transmission is left an open one, and we can only say that *if* time is required for transmission, the rate of travelling must be enormously great.

But in the solar system we have distances to deal with compared with which the dimensions of the earth itself on which we dwell, let alone those of a landscape, sink into insignificance. It is conceivable that in travelling over those vast distances, if it does travel at all, light might occupy lengths of time which would not be insensibly small, and which possibly might be put in evidence by some celestial phenomenon. Thus if the light of the sun were emitted by flashes, the sun would take the place of the thunder-cloud in our supposed observation, and the planets that of the objects in the landscape. Though there are changes going on in the sun, as we

now know, there are none of such magnitude and suddenness as to be available for such an observation. Nevertheless it was by celestial observations, of a somewhat different kind, that the finite velocity of light was first revealed.

This was done as long ago as in 1676 by Roemer, who showed that an inequality in the times of occurrence of the eclipses of Jupiter's satellites, which he had observed, was simply explicable on the supposition that light is propagated with a finite velocity. The mean motion of a satellite round the planet being accurately known, from observations extending over a sufficient time, the times of successive eclipses can be calculated. It was found that the eclipses happened earlier or later than the calculated times, supposing the epoch so chosen as to make the accelerations and retardations balance on an average, according as the earth and Jupiter were on the same side of the sun or on different sides, and the differences in the apparent errors in the times of occurrence of the eclipses at different times of the year were proportional to the differences of the distances of Jupiter from the earth at those times. The greatest difference in that distance is evidently equal to the diameter of the earth's orbit (supposing for simplicity the orbits of the earth and Jupiter circular and in the same plane), and to this corresponds

the greatest difference in the apparent errors of the times of occurrence, which amounts to about 16 minutes and a quarter. We learn therefore that if this be the true reason of the observed inequality, light takes 8 minutes and a few seconds to travel over the distance from the earth to the sun. To express the velocity in miles per second, we require to know the dimensions of the earth's orbit, or what comes to the same the solar parallax, which, being the ratio of the radii of the earth and the earth's orbit, serves to express the latter in miles since the former is accurately known. According to the value of the sun's parallax which was accepted till recent years, the velocity of light so determined came out about 192,000 miles per second. More recent determinations have reduced this to about 186,000 miles per second.

For nearly 50 years after Roemer's discovery, no other phenomenon was known which indicated that the propagation of light was other than instantaneous; but then Bradley made the very remarkable discovery of the aberration of light. It would occupy too much time to go fully into this, and I must content myself with giving you a general notion of it.

Suppose a person out on a perfectly calm day when rain was falling, and accordingly, on account of

the perfect calmness supposed, falling vertically. If the person were at rest, he would deem the rain to be falling vertically. But suppose he were carried horizontally along with a motion so smooth that he was unconscious of it. The rain though falling vertically would *appear* to him to fall in a somewhat slanting direction, as if it came from a point not exactly in the zenith, but displaced from it towards the point towards which the observer is being carried. And if instead of falling vertically the rain be falling in a slanting direction, it will appear to the observer, unconscious of his own motion, to slant differently; in fact, if we compound the velocity of the falling rain with a velocity equal and opposite to that of the observer, the direction of motion of the rain will *appear* to be that of the resultant velocity.

Now Bradley found that just the same thing takes place with regard to light. The earth in revolving round the sun moves at the rate of about 20 miles in a second, towards a point in the heavens lying in the plane of the ecliptic 90° in advance of the heliocentric position of the earth, or rather what would be 90° if the earth's orbit were strictly circular. The light which comes from any particular star appears to come from a place deviating from the true place of the star towards the point of the heavens towards which the

earth is moving. And as this point goes round the ecliptic in the course of a year, the apparent place of the star describes annually a closed curve, in fact, a small ellipse, round the mean position of the star. The *law* of the apparent displacement of the star is found to be what it ought to be on the supposition that the cause of it is what has been above explained, and the coefficient of the displacement gives the ratio of the velocity of light to that of the earth in its orbit. We are thus furnished with a second means of determining the velocity of light, and observation will show whether the two do or do not agree. The result is a remarkably close agreement when we consider on the one hand the difficulty of fixing on the exact moment of disappearance of a satellite which enters the shadow of Jupiter, and on the other the smallness of the displacement which constitutes aberration, amounting at a maximum to only 20 seconds and a quarter of angle, about the angle subtended by a six-inch rule at the distance of a mile.

It is to be noted that the unit of length in terms of which the velocity of light in both of these methods is primarily expressed is the radius of the earth's orbit, and in order to translate it into miles per second we require to know the solar parallax.

While I am on this point I may mention that in

the year 1849 Fizeau, by an extremely ingenious combination of apparatus, succeeded in determining for the first time the velocity of light by direct experiment. The resulting value was confirmatory of the two astronomical determinations, but was not at first considered as capable of being put in competition with them for accuracy, and no wonder, since the whole interval of time to be measured in Fizeau's experiment, under the circumstances in which it was performed, amounted to only about the one twenty thousandth part of a second. Subsequent determinations, however, first by Foucault, by a different method, and afterwards by Cornu, by Michelson, and by the late Dr James Young and Prof. G. Forbes, by one or other of those methods modified, have rendered the determination so certain and accurate that it is probably quite equal in accuracy to the astronomical determinations. And since the experimental determination gives the velocity in terms of a known distance on the earth's surface, and accordingly in miles or kilometres per second, if we assume the experimental and either of the astronomical determinations as separately valid and sufficiently accurate, by comparing the two we can determine the radius of the earth's orbit, which fixes the scale of the whole solar system, in miles or kilometres. And as we know accurately the dimensions of the earth, we can thus

determine the solar parallax, by combining accurate measurements of an astronomical phenomenon frequently recurring or constantly going on with a laboratory determination made once for all.

I have been tempted into a digression by the interest of this subject, and I would now resume the consideration of some of the elementary properties of light, with a view of showing how we are gradually led to the formation of a theory, now thoroughly tested, as to the nature of Light itself.

The observations to which I have last alluded show that light, whatever it may be, takes time to travel, so that in speaking of light as an influence that proceeds *from* a source of light there is no longer occasion to exclude the idea of motion of some sort. I have compared and contrasted two influences of which I have spoken, light and gravitation, and in this last point again it is a contrast with which we are presented; or at least if the two are analogous the analogy has never yet been established. After having ascertained that light takes time to travel, and contemplated that exceedingly curious phenomenon relating to light, aberration, the question naturally presents itself to the mind, Is there anything analogous as regards gravitation? Does it like light take time to travel, or is it an instantaneous influence? Now the consequences which would follow as regards

the motions of the bodies of the solar system if gravitation like light took time to travel have been calculated, and it has been concluded that if the influence of gravitation takes time to travel, it at any rate travels incomparably more quickly than light, with a velocity accordingly which is incomparably greater than 186,000 miles per second.

I have reduced to a minimum the mention I have made of the fundamental properties of light, in order not to weary you by repeating what is to be found in every text book, and we may now attack the question, What notion are we to form of the intrinsic nature of this agent, so important to our well-being, so wonderful in the scale of magnitude of the quantities it brings before us, as we have seen already as regards the velocity with which it is propagated, and shall see later on in other respects?

There appear to be but two modes possible of conceiving of a *mechanical* influence emanating from an influencing body, travelling with a finite velocity, and ultimately influencing another body at a distance. A mechanical influence implies the action of matter of some kind, and this matter we may suppose to have been either darted forth, in the manner of a projectile, or to have previously existed in the space between the influencing and the influenced body, to have been disturbed by the influencing body, and

then to be successively agitated in successive portions, each portion being agitated by its predecessor, and in its turn yielding to its successor the disturbance so received. We have not far to go to find illustrations of both these kinds of action. Bullets exemplify the first; the progress of waves at the surface of water illustrates the second. A still better illustration, except that it is not visible, is afforded by the phenomena of sound.

Such accordingly are the ideas which lie at the base of the two theories as to the nature of light which for a long time divided the scientific world between them, the corpuscular theory, or theory of emission, and the theory of undulations.

Prima facie there is much to be said in favour of the theory of emission. It lends itself at once to the explanation of the rectilinear propagation of light, and the existence of rays and shadows. It falls in at once with the law of aberration. The laws of reflection and refraction admit of an easy explanation in accordance with it; at least if we except the existence of both reflection and refraction; for according to this theory we should rather have expected beforehand that light would have been *either* reflected or refracted, according to circumstances, not that incident light should have *divided* into a portion reflected and a portion refracted.

The theory of undulations on the other hand presents at the outset considerable difficulties. In the first place it requires us to suppose that the interplanetary and interstellar spaces are not, strictly speaking, a vacuum but a plenum; that though destitute of *ponderable* matter they are filled with a substance of some kind, constituting what we call a *medium*, or vehicle of transmission of the supposed undulations. When I speak of this medium as a substance, or as material, I mean that it must possess that distinctive property of matter, inertia; that is to say a finite time must be required to generate in a finite portion of it a finite velocity. The necessity of thus filling space with substance seems to have presented a serious difficulty to some minds. In the course of a conversation with Sir David Brewster, who had just returned from France, where he witnessed the celebrated experiment by which Foucault had just proved experimentally that light travels faster in air than in water, I asked him what his objection was to the theory of undulations, and I found he was staggered by the idea *in limine* of filling space with some substance merely in order that "that little twinkling star," as he expressed himself, should be able to send its light to us.

I cannot say that this particular difficulty is one which ever presented itself as such to my own mind,

To me the difficulty is rather that of conceiving such an influence as that of gravitation to extend across an absolute void. Such was the feeling of the great discoverer himself of universal gravitation. In a letter to Bentley, quoted by Faraday as falling in with his own views, Newton thus expressed himself:—

"That gravity should be innate, inherent and essential to matter, so that one body may act on another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

If the supposition that light consists in undulations obliges us to suppose that space is filled with some kind of substance, at least as far as the remotest star that our most powerful telescopes reveal to us, may it not be that that same substance forms, in some manner as yet unknown to us, the link of connexion whereby the sun is enabled to attract the earth, and keep it in its orbit? It is true that notwithstanding the labours of various scientific men we are not in a

condition to give an explanation of gravitation, but our inability to explain it by no means proves that it is a primary property of matter, incapable of explanation, or forbids us to suppose that it may in some way be brought about through the intervention of that same substance which we find necessary to assume for the explanation of the phenomena of light on the theory of undulations. And it is quite conceivable, and we may now say even probable, that this same substance has yet other offices to fill. Perhaps the most remarkable of all the investigations of the late Professor Clerk Maxwell is that in which he showed that there is a certain velocity, numerically determinable by purely electrical experiments which can be made and have been made in the laboratory, and expressing the velocity of propagation of an electrical state, which is identical with the velocity of light within the limits of error of the experiments and observations whereby the two have been determined. Assuming for the moment, as a thing at the present day resting on evidence quite overwhelming, that light consists of undulations, we cannot fail to be impressed by the multiplicity of purposes, all bearing so intimately on our well-being, which it seems probable, or not unlikely, are fulfilled by one and the same substance, endowed with properties which we are only gradually learning.

I have ventured to allude for a moment to the present state of the theory of light, and I will now go back. The necessity of assuming the existence of some kind of substance in what we commonly speak of as a vacuum, does not appear to have been a serious preliminary difficulty in the way of the reception of the theory of undulations. A far more formidable difficulty appeared at first to be presented by the existence of rays and shadows. It was this that led Newton to adopt the theory of emission, though even he was led in the course of his researches on light to suppose that there was some sort of medium through which the particles of light moved, and in which they were capable in certain cases of exciting a sort of undulation. But the supposition of particles darted forth seemed to him necessary to account for shadows. If light consisted simply of an undulation propagated through a medium of some kind filling the interstellar spaces, as we know sound consists of an undulatory movement propagated in the air, how can we conceive of the existence of shadows, knowing as we do that sound passes freely round corners, and diverges after passing through apertures, though indeed it is true that the freedom is not absolute? Newton's contemporary Huygens was more bold, and adopted the theory of undulations pure and simple, rejecting altogether the notion of particles darted forth from

the luminous body, and travelling with the velocity of light. Huygens made a grand attempt to explain the existence of rays, the great stumbling-block at the threshold of the theory of undulations. The principle which goes by his name lies at the very foundation of the theory of undulations, and itself rests on a strictly mechanical basis, being in fact merely an application to the particular question under consideration of the general dynamical principle of the superposition of small disturbances. But this principle does not by itself alone suffice for the explanation of rays. It proves, or at least appears to prove, too much. It is as applicable to sound as, on the supposition that light consists in undulations, it is to light; and if Huygens's explanation of rays were complete there ought equally to be rays of sound, and sound ought to present the same sharp shadows as light.

Huygens attempted to get over this difficulty by entering on certain speculations as to the ultimate constitution of the *ether*, as we call the supposed medium which is the vehicle of light, and as to the mode of action, one on another, of the ultimate molecules of which he imagined it to consist. In this he abandoned the simplicity of the fundamental conceptions of the theory of undulations, and adopted a mode of reasoning not strictly allowable. For the transmission of regular undulations, of which the

period is arbitrary, at least within wide limits, requires us to suppose that the transmitting medium is either continuous or may be treated as such; that if it consist of ultimate molecules, or be otherwise heterogeneous, the number of intervals from molecule to molecule, or of deviations of one sign or another from an average homogeneity, shall be very great and as good as infinite within the length of a single undulation; and we have no right to extend to the medium treated as a whole, and regarded as continuous, a mode of communication of motion applicable only to the communication from one to another of a set of discrete molecules.

Accordingly, notwithstanding all that Huygens has done, the existence of rays and shadows, one of the most obvious properties of light, had received no satisfactory explanation on the theory of undulations such as it came from the hands of Huygens; and in this condition it remained for considerably more than a century. His explanation of the laws of reflection and refraction leaves nothing to be desired, except in so far as these laws involve the conception of rays. I cannot now speak of his discovery of the laws of double refraction in Iceland spar, because it belongs to a different branch altogether of the subject.

The theory of rays and shadows long remained in this unsatisfactory state; in fact, till quite the end of

the last century. Newton's discovery of the compound nature of white light showed that there must be in light an element of some kind susceptible of continuous variation. Each theory, the corpuscular and the undulatory, furnishes elements susceptible of continuous variation. What the element is, on the theory of emission, has not, so far as I know, been specified by the supporters of that theory, and difficulties seem to attend whatever supposition in that respect you can make. The theory of undulations presents one, and I may say but one, element which might serve for the purpose, namely, wave length, or what comes to the same thing, periodic time. That in fact periodic time must be the element variations in which correspond to variations in refrangibility, is clearly pointed out by other phenomena which I have not as yet touched upon. But this development of the theory did not take place till the present century, though some of the leading facts on which it is based were known to and studied by Newton. Accordingly from the time of Newton till the end of the last century, and even further, the theory of emission was that chiefly in vogue with scientific men. Various causes probably contributed to this result. The rectilinear propagation of light at first sight looks more like the motion of projectiles than the propagation of undulations, which in cases of what are

undoubtedly undulations spread out much after being laterally confined. The impetus given to the study of the motion of particles under the action of known forces by Newton's great discovery of universal gravitation, turned the labours of men of science into that channel rather than to a study of the propagation of vibrations. The great weight again of Newton's authority had doubtless its share in leading men to follow the theory as to the nature of light which he had taken up.

It is probably due to this preponderating influence of the theory of emission that so little notice was taken of the theory of the aberration of light. In the explanation of the phenomenon which is contained in the ordinary text-books of astronomy, which has doubtless descended traditionally from that given in earlier treatises, it is quietly assumed as a matter of course that the rectilinear propagation of the light coming from a heavenly body is not disturbed by the motion of the earth. Did light consist of particles darted forth, there is no reason to suppose that it should; in fact, to make such a supposition would be to fly in the face of all we know respecting the action of attracting forces, since any motion of the attracting body does not enter into account. But on the theory of undulations it is far otherwise. We should naturally have been disposed to look on the

earth in its motion round the sun as ploughing its way through the ether. Now if light consist of undulations propagated through this ether, we might have expected that the ether being pushed by the earth out of its way, the course of the undulations which it carried would be affected, possibly in an irregular way, in case eddies were produced, and at any rate in a manner which there appears no reason *a priori* should be in conformity with the simple law of aberration.

Accordingly Dr Young, to whom mainly we owe the revival of the theory of undulations which took place about the beginning of this century, supposed that instead of the earth's pushing the ether out of its way, it allowed it to pass freely through its substance, 8000 miles though it be in thickness, far more freely than a grove of trees transmits the wind; and that in consequence of this perfect freedom of passage, the ether outside the earth's surface was not disturbed by the earth's motion, nor consequently the undulations passing through it.

Now startling as is this supposition, and contrary to all that we should have anticipated, we cannot say that it must be rejected. For we must remember that we have no direct evidence even of the existence of an ether; it is not directly recognizable by any of our senses; its properties may be, and doubtless are,

very different from those of ponderable matter, and we must be content to learn them by degrees, as they may be revealed by the study of the phenomena which are referable to actions of the ether. Nevertheless we are not absolutely driven to accept Dr Young's hypothesis; for there is as I have shown another way in which the law of aberration may be obtained; a way which though not free from difficulties exempts us from the necessity of supposing that the earth in its motion through the ether allows the ether to pass through it with absolute freedom.

At this point it may be well to pause for a moment and consider the probabilities in favour of the two hypotheses. The existence of rays and shadows seems perfectly simple according to the theory of emissions; as far as we have gone it presents a serious difficulty on the theory of undulations. The laws of reflection and refraction leave little to choose between the two. In one respect indeed the theory of undulations has the advantage; for it indicates that there ought to be a partition of the incident light into a portion reflected and a portion refracted, whereas on the other we should rather have expected that the light would have *either* been reflected or refracted. This advantage is however of no great weight, for it would not be difficult to frame plausible hypotheses in the theory of emission which

would lead to a partition. As regards aberration, the corpuscular theory has a decided advantage, for on it the explanation of the phenomenon is perfectly simple, whereas according to the theory of undulations all we can say is that it is not inexplicable.

The balance on the whole seems to lean towards the side of the corpuscular theory. And yet that theory is now altogether exploded, and the rival theory is established on so firm a basis that no one who has studied the subject can doubt that the second of the two modes of conception with which we started expresses the truth, and that light really consists of a change of state propagated from point to point in a medium existing between the luminous body and that which the light affects.

It may be said, If the former theory is now-a-days exploded, why dwell on it at all? Yet surely the subject is of more than purely historical interest. It teaches lessons for our future guidance in the pursuit of truth. It shows that we are not to expect to evolve the system of nature out of the depths of our inner consciousness, but to follow the painstaking inductive method of studying the phenomena presented to us, and be content gradually to learn new laws and properties of natural objects. It shows that we are not to be disheartened by some preliminary difficulties from giving a patient hearing to a hypo-

thesis of fair promise, assuming of course that those difficulties are not of the nature of contradictions between the results of observation or experiment and conclusions certainly deducible from the hypothesis on trial. It shows that we are not to attach undue importance to great names, but to investigate in an unbiased manner the facts which lie open to our examination.

I now come to other classes of phenomena with respect to the explanation of which there is the widest possible difference between the two hitherto rival theories respecting the nature of light. We are all familiar with the vivid colours of soap bubbles, colours which are exhibited in the case of transparent solid plates or liquid films which are excessively thin, or of very thin plates of air contained between two surfaces of glass which are in contact, or almost in contact, at one point. It is to Newton that we owe the first investigation of the laws of these "colours of thin plates" as they are called. By placing a convex lens of small curvature in contact with a plane piece of glass, we obtain a separating plate or film of air the thickness of which vanishes at the point of contact, and increases very slowly at first on receding from it. In this case the colours are arranged in circles round the point of contact, the rings forming annuli of increasing diameter but decreasing width as we

recede from the centre outwards. The centre is dark; and when the incident light is white, on going outwards about seven alternations can be traced, after which the field is sensibly of uniform illumination and free from colour. The squares of the diameters of the rings are found to increase in arithmetic progression in passing from ring to ring.

When instead of using white light the system of the rings is illuminated by the colours of a pure spectrum, a vast number of rings is seen; in fact, they go on till from the increasing narrowness of the annuli they become too fine to be seen. In this case the colour naturally remains the same, being that of the part of the spectrum that is used to illuminate the glasses. The scale of the system changes greatly with the colour, decreasing from the red to the violet.

The scale of the system depends very greatly upon the curvature of the lens; and to form the rings on a large scale, which is convenient for examination, it is necessary to use a lens of small curvature. The thickness of the interposed plate of air where any ring is formed can be obtained by an easy calculation from the diameter of the ring and the radius of curvature of the lens, both which can be measured. When this is done it is found that a given ring is always formed where the plate has a given thickness.

If the interposed medium be water instead of air,

or generally any transparent fluid, the scale of the rings is diminished. It is found that the square of the diameter of a given ring varies inversely as the refractive index of the interposed liquid.

As my object is not to give a complete account of the phenomena of the rings, much less a complete explanation of the phenomena, I shall dwell no further on the appearances which the rings present under different circumstances, the features I have already described being sufficient for my purpose, which is to give a fair idea of the evidence on which rests that theory as to the nature of light which we shall be led to adopt.

Newton endeavoured to account for the various phenomena of thin plates by his celebrated theory of fits of easy reflection and transmission. We have seen that the existence of both reflection and refraction is a difficulty in the theory of emission which has in some way to be accounted for. It will not do to suppose that there are two permanently distinct kinds of light, of which one, when the light falls at a given inclination on a given substance, is always reflected and the other always transmitted, for if either the reflected or the transmitted beam be allowed to fall at the same angle on the same substance, it is divided into a reflected and a transmitted beam. We must therefore suppose, even independently of the phenomena of thin plates, that the same particles of light are sometimes in a

condition to be reflected and sometimes in a condition to be transmitted: and we have only further to postulate that these changes of state, whatever may be their nature, take place with a regular periodicity in order apparently to account for the phenomena of thin plates, at least if we restrict ourselves to the case of a perpendicular incidence on a plate of given kind, such as a plate of air between two surfaces of glass. For all the light which is transmitted by the first surface must be in a fit of easy transmission which it does not at once lose, so that if it falls immediately on the second surface it is transmitted, and therefore the central spot looks comparatively black. The same must be the case where the distance between the surfaces is equal to the length of a fit, or 2, 3, &c. times the length of a fit; and for intermediate thicknesses of the plate, corresponding to intermediate distances from the point of contact, the effect will be intermediate, and there will be more or less reflection, which will be greatest for thicknesses exactly half way between the critical thicknesses above mentioned.

But tempting as this theory at first sight appears, though if it be true it must be left to subsequent research to indicate what that periodic element in relation to the particles of light can possibly be which shows itself by an alternate capacity for reflection and refraction, it fails completely to account

for the other features of the phenomenon, some of which I have mentioned. We should have expected beforehand that the length of a fit would have been independent of the angle of incidence; and when we learn that to reconcile theory and observation we must *suppose* it to vary as the secant of the angle of incidence, we see no way of accounting for such a law.

Again, as to the effect of a change of medium, the most natural supposition to make would be that the constant element of periodicity which characterises light of any particular refrangibility is a constant periodic time. Now the explanation of the law of refraction according to the theory of emission requires us to suppose that light travels faster in refracting media than in vacuo, in the ratio of the refractive index to unity. We might have expected accordingly that the length of a fit in water would be greater than in air in that ratio. We have seen however that it is less in the inverse ratio. We are unable to frame any plausible hypothesis, on the theory of emission, why it should be so.

Accordingly the theory, if such it can be called, consists merely of a set of incoherent laws, not indicated beforehand by theory, not even falling in with it after they have been pointed out by observation, and it has accordingly nothing about it which seems to bear the stamp of truth.

Nor is this all. Even as regards the formation of the rings at a perpendicular incidence, where at first sight the theory appears to be most successful, it leads to a conclusion which is belied by observation. According to this theory, the office of the first surface of the plate is solely one of sifting, by reflecting back those particles of light which are in a fit of reflection, and thereby preparing the way for the alternate transmission and reflection of the particles at the second surface. There should therefore be as much light reflected at the first surface as if the second glass were away altogether, and therefore the dark rings should be only comparatively dark. The experiment may be easily and successfully tried with homogeneous light, such as that of a spirit-lamp with salt on the wick, and it is found that the dark rings are decidedly darker; in fact as to sense apparently black.

LECTURE II.

Interference—Explanation of the colours of thin plates afforded by the theory of Undulations—Diffraction.

IN my last lecture I pointed out the insufficiency of the theory of emission to account for the various phenomena of the colours of thin plates. Let us now see whether the theory of undulations lends itself to an explanation. In the first place the element of periodicity, instead of being wholly extraneous to the fundamental idea, and hardly if at all to be reconciled with it even when suggested by phenomena, is one naturally, almost inevitably, involved in the fundamental conception. I say, *almost* inevitably; for a set of undulations *might* consist of a succession of isolated pulses following one another in a wholly irregular manner; but the analogy of sound would make it far more likely *a priori* that they should form a series of regularly periodic disturbances; and the supposition that such is their character therefore harmonizes perfectly with what we should have expected beforehand.

Supposing then that the undulations we have to deal with are regularly periodic, can we on the undulatory theory give any account of the alternations of light and darkness which we observe in Newton's rings?

The explanation which this theory affords is based on a very simple and very general dynamical principle, of very wide application, called the principle of the superposition of small motions. For the sake of those who may not have much attended to dynamics, I will endeavour to give some idea of what this principle means.

Suppose a stone thrown into still water. It produces as we know a series of small waves, which spread out in the form of circles from the place of disturbance. What passes outwards is, not the material particles of the water, but a certain state of things. If we observe a minute floating body, it is seen to retain its average position, and merely to move very slightly backwards and forwards, up and down. So far, we have merely a visible illustration of the progress of an undulation, differing it is true from those belonging to sound or light in the fact that it is only in the neighbourhood of a particular surface that the disturbance is sensible, which does not however prevent it from forming a useful illustration for those to whom the conception may be new.

But now instead of a single stone, suppose that

there are two similar stones thrown in simultaneously at a little distance apart. Each will give rise to a series of circular waves which at first will be distinct, each diverging from its own centre, but not yet having reached one another. But presently the waves from the one centre will invade the region occupied by those from the other. What will then take place? According to theory, each disturbance will find the mass in which it is being propagated under as nearly as possible the same conditions, so far as itself is concerned, as if the other disturbance did not exist. The consequence is that any particle of the mass will be as much displaced from its mean position as if the other disturbance did not exist, and its actual place will therefore be found by compounding, as it is called, the displacements due to the two disturbances taken separately. Accordingly where a ridge due to one of the disturbances coincides in position with a ridge due to the other, we have an elevation of double height, supposing we choose a place where the intensities of the disturbances due to the two sources are the same; but where a ridge due to one falls in with a trough due to the other, we have neither elevation nor depression, but the water is found at its natural level.

Now if there be any truth in the theory of undulations, something of the same kind must take place

with light. If we have two sets of disturbances always exactly alike, then when the conditions are such that the two disturbances separately considered take place always in opposite directions, being superposed they simply produce a disturbance *nil*, whereas when the directions agree they produce a disturbance of greater amount than either separately. And if light consist of such a disturbance in the ether, then in the former case we ought to have no light, whereas in the latter the light ought to be greater than if the one set of disturbances alone existed. And if the two sets of disturbances, while still in other respects alike, differed in amplitude in a constant ratio, the only difference that would make in the result would be that the light would not vanish at the minima.

But it must be particularly remarked that in order that this neutralization should always take place in the same way at the same place, it is essential that the two sets of disturbances should always agree as to their times of starting, or differ, if they do differ, by a perfectly constant quantity. For if one were half an undulation ahead of the other, that would make all the difference whether the two disturbances strengthened or neutralized each other: and if the relative times of starting varied irregularly a vast number of times in a second, we should have at a given place neutralization and cooperation succeeding

one another so rapidly that nothing but the mean effect would be perceived, and that, as may be shown, would be simply the sum of the mean effects taken separately. Now if the disturbances came from two independent sources, such as two different portions of a flame, the relative starting points of the two would be purely casual, and no fixed and permanent neutralization would be to be expected. And observation shows that none of those alternations of light and shade which on the theory of undulations we refer to interference are manifested unless the two interfering streams of light have come originally from the same source, having subsequently pursued slightly different paths.

Let us now see how these principles apply to the explanation of Newton's rings. If we consider a small portion of the thin plate of air by reflection from which they are seen, we perceive that there are two reflecting surfaces near one another, and consequently two reflected streams, one reflected from the upper surface of the plate of air, that is, internally in the upper glass, and the other reflected from the under surface of the plate of air, that is, from the upper surface of the under glass. These are it is true accompanied by other streams which have been reflected backwards and forwards internally in the plate, so that the total number of reflections in these

streams is 3, 5, 7, 9...; but these streams, being usually comparatively weak, may in a general explanation be left out of consideration. Now the second of these streams has had to travel a very little further than the first. In the simplest case, to which we may confine ourselves for the present, that of a perpendicular incidence, the excess of length of path of the second stream is evidently just double the thickness of the plate of air, and varies accordingly from point to point of the field of view, the thickness increasing slowly at first and afterwards rapidly as we recede from the point where the lenses are in contact. Close to the point of contact, the second stream is only imperceptibly behind the first, and the two might be expected to be in accordance, that is, to agitate the ether in the same direction, and therefore to strengthen one another. As we take a point further outwards from the point of contact, the retardation increases, and when it becomes half the length of a wave the two would be in opposition, that is, one would agitate the ether in a way just opposite to the other, and therefore, supposing the intensities of the two the same, as would nearly if not exactly be the case, they would neutralize each other, and darkness would be the result. And as the thickness of the plate of air between the two glasses varies as the square of the distance from the point of contact, the

law of increase of the rings for any one kind of light as the order of the ring increases follows at once, and moreover the law that under different circumstances as to curvature of the glasses the same ring is always formed where the thickness of the plate is the same.

But there is one marked point of contrast between the results of theory, so far as I have yet explained it, and observation, namely, that we have been led to expect a maximum of brightness where the paths of the two streams are the same, that is close to the point of contact, or where they differ by one, two, or a complete number of undulations, and a maximum of darkness if not absolute blackness where the difference is an odd number of half undulations. This would give the places of light and shade exactly reversed as compared with observation, which would show one of two things, either that the theory must be rejected, or that some circumstance has been overlooked which, had it been taken into account, would have made the theoretical result right in this respect.

Now the reflections which the two streams respectively undergo take place under very different circumstances. The first stream is reflected internally in glass when the light, which had previously been travelling in the glass of the upper lens, arrives at the under surface of the glass, or upper surface of the interposed plate of air, the second is reflected back

into air at the upper surface of the under lens. As the two reflections take place under different, in some respects opposite, circumstances, there seems nothing unlikely *a priori* in the supposition that the signs of the reflected vibration should be opposite in the two cases. Dynamical analogies are not far to seek; for example, we know that when sound travelling along a pipe meets a closed end, it is reflected in such a manner that condensation in the reflected answers to condensation in the incident, and rarefaction to rarefaction; but when it is reflected, as reflected it is, from the open end of a tube, rarefaction answers to condensation and condensation to rarefaction. And this theoretical conclusion as to the opposition of signs in the two cases, so probable from analogy, is converted into a certainty by the application of a simple dynamical law of great generality, which may be called the law of reversion. Hence then the occurrence and features of the rings at a perpendicular incidence, for any one kind of light, are on the undulatory theory matters of pure prediction.

Next consider the effect of inclination. It follows from a very easy calculation that the effect of the thickness of the plate of air at any inclination in producing retardation in the stream reflected from the second surface of the plate, relatively to the stream reflected from the first surface, is the same

as it would be at a perpendicular incidence for a thickness smaller in the proportion of the cosine of the inclination of the light while in the plate of air to unity, or, which is the same thing in the case of a lens with nearly parallel surfaces for the upper glass, of the cosine of the angle of incidence on the first surface to unity. Hence follows at once the simple law of dilatation of the rings on increasing the angle of incidence, namely, that the square of the diameter of any ring varies as the secant of the inclination on the first surface. Of this law, as we have seen, the theory of emissions could give no account.

Again, the effect of the substitution of water for air between the lenses in causing the rings to contract, and the law of their contraction already mentioned, follow immediately on this theory from first principles, since the explanation of refraction on the theory of undulations necessitates the supposition that light travels more slowly in refracting media than in vacuum, in the proportion of the refractive index to unity.

In short, the theory completely explains the phenomena of the rings seen by reflection. And this is true even as regards the more minute features, into which I have refrained from entering. The Newtonian "fit" at a perpendicular incidence expresses half the length of a wave; and as the scale of the

rings decreases from the red to the blue, and accordingly as the refrangibility of the light increases, we learn that the variable element in light which corresponds to a change of refrangibility and change of colour must be the wave-length *in vacuo*, or, what comes to nearly the same if not exactly the same thing, the periodic time. The compound tints of the rings are explained here as in all other optical phenomena, whatever theory of light we may adopt, by the superposition of the ring-systems corresponding to the different kinds of light of which white light consists, which are on different scales as to size; and the fact that not more than about seven rings are seen with white light merely depends on the overlapping of the rings of high orders corresponding to the different colours.

I have hitherto said nothing about the system of rings in the transmitted light, which are complementary in character to the rings seen by reflection, but far less vivid, resembling in fact a vivid system complementary in character to the reflected system, overlaid by a comparatively large quantity of uniform white light. In nearly every respect the theory of these is analogous to that of the reflected system, so that whatever theory explains the one can hardly fail to explain the other. There is just one feature the explanation of which involves considerations into

which I have not entered, and to this I will confine myself.

The transmitted system, according to the explanation afforded by the theory of undulations, depends on the interference of two streams of light, one passing right through the plate of air comprised between the lenses, and the other following it after two reflections in air at the adjacent surfaces of the glasses. These are associated with other portions which have been reflected 4, 6 ... or any even number of times; but these, being usually comparatively weak, may in a general explanation be disregarded.

Now when light is incident perpendicularly, or nearly so, on the surface of crown glass, about 4 per cent. of the incident light is reflected, and the rest enters the glass. The intensity of light once reflected in this manner being thus only the $\frac{1}{25}$th of the intensity of the original, the intensity of light twice reflected will be only the $\frac{1}{25}$th of that, or the $\frac{1}{625}$th of the original. Now as the light which passes through one surface loses only 4 per cent. by reflection, the light which passes through the two surfaces of the plate still retains the $0{\cdot}96^2$ or $0{\cdot}9216$ of its original intensity; so that the twice reflected light has an intensity only the $\frac{1}{576}$th of the direct, hardly more than one-sixth per cent. How then, it might be said,

could so small an addition or subtraction of light be perceptible at all, and so produce in homogeneous light differences of intensity, and with white light changes of colour, which though not it is true by any means so striking as in the reflected system of rings are nevertheless very evident, and demand far greater differences of intensity than that?

The explanation of this paradox lies in a consideration of the relation between intensity and amplitude of vibration. If light consists in a disturbance of a subtile medium, or ether, then the greater be the disturbance, other circumstances being the same, the stronger must be the light. But supposing the amplitude of excursion of the ether to be doubled, trebled... is the intensity of the light doubled, trebled ... or if not, in what other proportion is it increased?

Now a number of different considerations lead decisively and independently to the same conclusion, namely, that the intensity is to be measured by the square of the amplitude of excursion. Hence if the amplitude of excursion to and fro of the ether is increased in the proportion of 1 to 2, 3..., the intensity of the light is increased in the proportion of 1 to 4, 9...

Suppose now that two disturbances from the same source, and following nearly the same path, have *a, b* for the coefficients of excursion due to them sepa-

rately; when acting together the coefficients of excursion will fluctuate between the limits $a+b$ and $a-b$ according as the phases are in agreement or in opposition. Hence whereas the intensities of the two separately will be in the ratio of a^2 to b^2, the maximum and minimum intensities due to the compound disturbance will be proportional to $(a+b)^2$ and $(a-b)^2$.

Suppose now the first disturbance to be very much greater than the second, then a will be very much greater than b. Hence whereas the sum or difference of the intensities of the two streams would be got by adding or subtracting b^2 to or from a^2, the maximum and minimum intensities of the compound disturbance will be got very nearly by adding or subtracting $2ab$, a quantity which is greater than the former in the proportion of $2a$ to b. Thus with the numerical values just mentioned, which are applicable to Newton's rings at a perpendicular incidence, if we take the coefficient of vibration and the intensity of the stronger stream each for the unit of their respective kinds, the coefficient of vibration of the weaker stream will be $\frac{1}{25}$ and the intensity $\frac{1}{625}$ nearly, whereas the maximum and minimum intensities of the compound stream will be $1 \pm \frac{2}{25}$ nearly, and the difference between them will be as great as $\frac{4}{25}$.

In the particular case of Newton's rings, the weaker stream is not readily viewed apart; but in

certain experiments of diffraction, a subject that will be touched on by and by, the two streams lend themselves readily to separate observation, and the occurrence of distinct fringes of interference theoretically referable to an invisible agent—invisible because too faint to be seen—is not a little paradoxical in appearance.

It appears then that the fundamental hypotheses of the undulatory theory suffice to account in the most complete manner for the phenomena of Newton's rings, and the colours of thin plates in general, without making any fresh assumption whatsoever. It is to Dr Young that we owe the explanation of these colours on the theory of undulations, an explanation given at a time when any other theory than the corpuscular could hardly gain a hearing: and to him also we owe the first direct experiment proving that certain fringes which connect themselves by numerical relations with Newton's rings are incontestably due to interference.

As introductory to this experiment, it will be convenient to mention another phenomenon which forces itself upon the notice of the observer simultaneously with that with which we are more immediately concerned, and which therefore it is desirable to be able at once to refer to its proper place, besides that at a more advanced stage of our study of the

subject it will be found to be of very great importance.

Suppose that the sun's light is reflected horizontally into a darkened room, passing through a very small hole in the shutter, or what is more convenient through a lens of short focus. Let the light be allowed to fall on an opaque screen at the distance say of a few feet from the luminous point, the screen being terminated by a straight edge, suppose vertical. Let the light passing the edge of the first screen be received on a white vertical screen which we may suppose a few feet further off from the luminous point. According to geometrical optics, if we project the edge of the opaque screen on to the receiving screen, by straight lines drawn from the luminous point, all to the illuminated, suppose the right-hand, side of the projection we shall have uniform illumination, the same as if the opaque screen were away, while all to the left of it we shall have darkness. According to observation, there is no such abrupt transition on the receiving screen from uniform darkness to uniform brightness. The illumination increases continuously, though rapidly. The illumination begins to be sensible before we reach the geometrical shadow, or projection of the straight edge, where it is still considerably feebler than the full illumination at a distance on the right. On going in

a right-hand direction from the geometrical shadow the illumination rapidly increases, and actually becomes considerably greater than the full illumination at a distance. It then decreases to a minimum, increases to a maximum, and so on, the maxima and minima differing less and less, by excess and defect respectively, from the illumination at a distance, and gradually occurring in more rapid succession, till they become insensible. Three such bands can usually be traced before the illumination becomes sensibly uniform. With white light the scale of the bands, and of their distances from the geometrical shadow, changes as usual from colour to colour, decreasing from the red to the blue. The appearance is the same at all distances of the two screens from the luminous point, the scale, merely, of the system varying according to circumstances, and likewise the smallness demanded in the source of light in order that the bands may not be confused. From the constant character of these bands we readily recognise them in experiments in which they appear associated with other phenomena.

Suppose now that instead of the opaque screen extending indefinitely on one side, the light is intercepted by a narrow slip, a knitting-needle, or anything of the kind. The shadow is bounded on both sides externally by fringes of the character of those

just described, and which accordingly we must attribute to the same cause, referring the right-hand set to the light which passed to the right of the obstacle, acting independently of that which passed to the left, and similarly as regards the external fringes of the left. But besides these the shadow itself is occupied by another set of coloured fringes, finer usually than the former, and unlike them of equal width throughout. They may be viewed through an eye-lens; and thereby magnified. If the screen be semi-transparent, we may view them with the lens from behind, and now if we take away the screen altogether, using nothing but the lens, and receiving the light directly into the eye, we see them as before. But now they are much brighter, since the light is no longer scattered in all directions by a screen, so that we can afford to use a narrower source of light, suppose a lens of shorter focus in the window; and having the narrower source we may use a wider opaque slip without the fringes getting confused. Under these circumstances the system of internal fringes occupies only the middle portion of the shadow, being well separated from the external fringes on both sides. The very centre of the shadow is bright for all the colours of the spectrum, and the middle bright band is accordingly white, except just at the edges, where it is slightly reddish, but after that the bands right

and left soon become coloured, on account of the difference of scale for the different colours.

Now what account can we give of the formation of these internal fringes on the theory of undulations? We have seen that when light passes by an opaque screen, it does not at once become insensible within the limits of the geometrical shadow; a little light bends round the edge into the darkness, enough to produce a feeble and rapidly decreasing illumination on a screen placed to receive it. This "inflexion" of light is a fact, account for it as we will: and if light consists of undulations, the analogy of those undulations which we can directly examine would lead us to infer that light must be inflected into a shadow. Indeed the grand original difficulty which for so long a time prevented the reception of the theory of undulations was that of accounting for the comparative absence of inflexion; in other words, for the existence of rays and the sharpness of shadows. Admitting this small inflexion as a fact, we see that at any rate we can not be far wrong in supposing the bending to take place close to the edge of the obstacle. On this supposition, the length of path of the inflected stream in travelling from the luminous point to the point of the field, or focal plane of the eye-lens, where we seek the illumination, will equal the path from the luminous point to the edge of the

obstacle, *plus* the distance from thence to the point in the field. The difference of paths for the two inflected streams which reach the same point of the field is accordingly easily found, especially as we are only concerned with points lying but a very small distance from the plane passing through the luminous point and the middle of the obstacle, or say the central plane. The difference of path vanishes at the central plane, where accordingly we have a maximum of brightness, and on receding in a lateral direction from the central plane the difference of path changes in proportion to the lateral distance, and accordingly it is equal to 1, 2, 3... wave-lengths for a series of equidistant straight lines parallel to that in which the central plane cuts the field. These lines are what ought according to theory to be the middle lines of bright fringes, and lines midway between them ought to be the middle lines of dark bands separating the fringes. The theoretical breadth of a fringe, being sensibly independent of the distance of the luminous point, is connected with the wave-length by a very simple formula, involving only the distance from the opaque slip to the screen on which the light is received and the breadth of the slip, both of which can easily be measured. On the other hand, the fringes are actually seen, and their breadths can be measured. Comparing the theoretical and observed breadth of a

fringe, we obtain the value of the quantity which if the theory be true expresses the length of a wave. On comparing this with the measure obtained from Newton's rings, we obtain identically the same value within the limits of errors of observation.

This numerical relation indicates that the alternation of light and dark in these two different phenomena is referable to a common cause, whatever that may be. Newton endeavoured to explain the rings which go by his name by the theory of fits of easy reflection and transmission. We have seen that this theory failed to account for the dilatation of the rings produced by increasing the angle of incidence, and for their contraction produced by the substitution of water for air as the interposed medium. Still, it gives correctly the law of increase of the radii of the rings at a perpendicular incidence as the order increases, and the law connecting the scale with the curvatures of the lenses. Now confining ourselves to the case of a perpendicular incidence, where the theory of fits is most successful, we may notice one radical difference between the explanations offered by the theory of fits and the theory of undulations. According to the former, the office of the first surface of the thin plate is simply one of sifting, and it is by reflection or non-reflection from the second surface that the rings are formed. According to the theory of undulations on

the other hand, there is no sifting at all: the light reflected from and the light transmitted by the first surface have both one and the other just the same properties as the incident light, and it is by the simultaneous working of the light reflected from the upper and that reflected from the under surface that the alternations of illumination are produced. Now in the experiment of the internal fringes, there is absolutely nothing to sift the light, and yet we get alternations of light and darkness as in Newton's rings, and what is more are conducted to the very same measurable length, clearly representing something inherent in the nature of light, which according to the theory of undulations is simply the length of a wave, the conception of which is radically involved in the fundamental points of the theory. We can hardly refuse to admit that the alternations of light and dark witnessed in these two phenomena are really due to interference; to the simultaneous working of two portions of light.

This conclusion was converted we may say into a certainty by a celebrated experiment of Dr Young's on the internal fringes. He showed that if a small opaque screen were placed so as to intercept the light going to fall on one side of the narrow slip, or else which had already passed it, in either case the central fringes disappeared as well as the external fringes on

the same side of the slip. And if a plate of glass were substituted for the opaque screen the internal fringes disappeared as before, but now the external fringes were seen on both sides. It is clear therefore that the internal fringes are really due to the joint working of the two portions of light inflected in passing the two edges, while as regards the external, those which appear on the right and left are independently produced by the light which passes on the right and left respectively of the opaque slip. The disappearance of the internal fringes occasioned by the interposition of a piece of glass on one side only of the opaque slip is explained in the same way as the non-exhibition of Newton's rings, when white light is used, outside a very moderate number surrounding the point of contact of the glasses.

After what precedes, the reality of the interference of light might well be taken as established. It is to be noted however that in the theory of the last experiment we have had to take for granted the inflexion of light. It is true that the fundamental conceptions of the theory of undulations lead us to expect inflexion, the difficulty being rather to explain how there is so little. We have not however as yet seen how the determination of the inflexion is to be brought within the domain of theory, and to that extent we have been working in a field not yet fully explored.

It is to Fresnel that we owe the first experiment of interference in which there is neither a thin plate, the first surface of which might according to Newton's views have exercised a sifting action on the light, nor anything unusual, such for example as inflexion, occurring in the progress of the light, but in which the two portions of light that interfere are simply regularly reflected or refracted as the case may be.

This object was accomplished by Fresnel in two ways, by reflection and by refraction: by a pair of interference mirrors, and by an interference prism, which is a prism with a very obtuse angle, such as 179°. It will be sufficient to mention the former method.

Let two plane mirrors be procured, such as two pieces of plate glass, blackened on the back, each mirror being bounded by a straight edge. Let them be mounted so that the straight edges are close together, and the planes of the mirrors are nearly but not quite continuations of each other; the planes of the faces making with each other a very obtuse angle, suppose only a fraction of a degree less than 180°, the concavity being on the side of the reflecting faces. Great care must be taken that neither mirror juts out above the other where they meet along the straight edges, which may be ascertained by passing the finger lightly across the junction. Let the sun's rays be reflected into a darkened room, and brought

to a point, or what may be regarded as such, by a lens of short focus. Let the light proceeding from the luminous point be received, say at the distance of a few feet, on the pair of mirrors. According to geometrical optics, the light reflected from each mirror will proceed after reflection as if it came from the virtual image of the point in the mirror. If each mirror be projected into space by lines drawn from the virtual image belonging to it, we see that there is a narrow wedge of space within which both reflected streams mix. If the light be received on a screen there will be two illuminated areas corresponding to these two projections respectively, and a narrow more highly illuminated band where the two overlap, corresponding in fact to a section, by the plane of the screen, of the wedge above spoken of. If this doubly bright portion of the field be more carefully scrutinized, by removing the screen and receiving the light directly into the eye through an eye-lens, we see the field marked on both sides by the usual external fringes at the boundary of an illuminated field. But besides these we see, running parallel to these along the middle of the field, a set of sharply defined, and commonly much narrower fringes, which as usual are on a different scale for the different colours, coarser for the red, finer for the blue. These are quite different in appearance from the compara-

tively vague external fringes which are seen at both sides of the field. And if any suspicion were entertained that after all these sharply defined fringes, said to be due to the interference of two regularly reflected streams, were really connected with the external fringes, their complete independence would be shown by a very simple modification of the experiment. Adjust the mirrors so that one shall jut out a very little above the other towards one end of the line of junction, and the other towards the other, this will be sufficient to make the line of intersection of the planes of the mirrors altogether oblique to the line of junction. The region of space within which the two streams mix remains as before, and the external fringes which bound it on the two sides, but the sharply defined fringes which before ran along the middle of the doubly bright region, being parallel to the line of intersection of the planes of the mirrors, are now altogether oblique to the line of junction, and accordingly inclined at a considerable angle to the external fringes. When the sharply defined fringes begin to run out of the doubly bright portion of the field, they and the external fringes begin to modify one another, but under the conditions which usually prevail, and at any rate are easily obtained, the former pursue for a long way their oblique course quite undisturbed.

It is proved therefore to absolute demonstration that the property of interference is one essentially belonging to light from its very nature. Two lights of the same kind, that is, of the same refrangibility, from the same source do really strengthen or oppose each other, in the latter case producing darkness if the intensities are equal, according as their lengths of path from the source to where they mix are the same or differ by a multiple of a certain length depending on the nature of the light, or as they differ by an odd multiple of half that constant. And if part of the path lies in some refracting medium, as water, instead of air, it is equivalent, so far as interference is concerned, to a path in air greater in the proportion of the refractive index to unity, and which accordingly would take exactly the same time to be travelled over according to the theory of undulations, since according to that theory the velocity of propagation in media is less than in vacuo in the proportion of unity to the refractive index.

This fundamental constant, which according to the undulatory theory expresses the length of a wave, may be determined more or less accurately by any of the instances of interference above mentioned; though the progress of our knowledge has furnished us with methods of determining it of still greater exactness. It increases as we have seen, and that considerably,

something like in the proportion of 2 to 3, in passing from the blue to the red. As to its absolute value, it will be sufficient to say that for rays of mean refrangibility we may take it in round numbers at the $\frac{1}{50000}$th part of an inch.

We have seen with what admirable simplicity the theory of undulations explains the various phenomena, in all their details, which have been mentioned as referable to interference. And yet the grand original difficulty, that of explaining the existence of rays and shadows, has been left untouched, or received at most only a lame explanation. Yet if the theory be true it ought to be capable of accounting for these phenomena as well as for those, in some respects simpler in character, which have been so successfully referred to interference.

It is remarkable that it was not till the study of the theory of light had made great progress, subsequently to the revival of the undulatory theory about the beginning of the present century, that the elementary phenomena of rays and shadows received their full explanation; and yet, once that explanation is propounded, it is seen to involve nothing more than the very elements of the theory of undulations; to be in fact nothing more than might have been foreseen from the beginning had the human race been sufficiently acute. The history of the

explanation compared with our present knowledge affords a remarkable example of the manner in which we start on our course of investigation by mounting on the shoulders of our predecessors; and so it may very likely be in the future that things that appear to us mysterious, and which we labour hard to explain, will to our successors seem so simple that they will wonder why we did not find them out.

I have mentioned the external fringes seen on the illuminated side of the geometrical projection of a straight edge bounding an opaque body exposed to light coming from a luminous point, which is taken as the point of projection. This phenomenon is modified in a great variety of ways according to the outline of the opaque body. It may for instance be a screen containing one or more apertures. Suppose the aperture circular. If the circle be moderately large, and the light be received on a screen beyond, we have a circular illuminated patch corresponding nearly with the projection of the hole, which is seen to be fringed within its boundary by fringes resembling the external fringes spoken of just now. If the size of the hole be diminished, or the distance of the receiving screen increased, which produces a similar effect, the fringes invade what had previously been the uniformly illuminated area corresponding to the projection of those portions of the hole which are at a little distance

from its edge; and after a very curious and complicated set of changes we are ultimately, when the hole is made very small, left with a circular patch of rather weak light on the screen, surrounded by a dark ring, followed by other rings alternately bright and dark, but of rapidly decreasing intensity, till they are lost in the dark shadow of the screen in which the hole is pierced. The central bright patch is much larger than the geometrical projection of the hole.

The continuity of the phenomenon connects the diffusion of the light which passes through the hole in the last case, and the comparative absence of diffusion in the first case, with the formation of coloured fringes and alternations of intensity about the boundary of shadows in general, and with the external fringes belonging to a straight edge in particular, and makes it probable that if we could explain these last the principles of the explanation would enable us also to explain the existence of rays, at least in so far as rays have a real existence at all.

Can we then explain the external fringes in the simple case of a straight edge? Newton made careful observations of the phenomenon; but guided as he was by ideas belonging to the corpuscular theory of light, he advanced no further towards an explanation than a few vague conjectures. Dr. Young, guided by the theory of undulations, was more successful. He

attributed these fringes to the interference of two portions of light, one coming direct from the luminous point, and one reflected at a grazing incidence at the edge of the opaque screen, losing half an undulation at the reflection. This theory explained very well the leading features of the fringes, showing them to be hyperbolic in form, that is to say that a section of the fringes, conceived as existing in space, made by a plane passing through the luminous point and perpendicular to the edge, is a system of hyperbolas having for their common transverse axis the line joining the luminous point with the edge, and having small conjugate axes, differing from one fringe to another of the system. It explained also the decreasing width of the fringes as we recede from the shadow, and their dilatation when the luminous point approaches nearer to the opaque screen from the edge of which they start. It even gives very nearly the breadths of the fringes and their distances from the geometrical shadow.

Accordingly when Fresnel, many years later, commenced his celebrated researches on diffraction, he in the first instance adopted Young's theory as to the cause of the formation of the external fringes. In the course however of his study he met with phenomena of diffraction which did not fall in with Young's view, and which at last opened his eyes to perceive the

grand principle which underlies the whole. We have seen that Huygens successfully explained the laws of reflection and refraction on the undulatory theory by introducing the principle that each element of the front of a wave may be regarded as the source of an elementary disturbance, and these disturbances must then be joined together. Now we have only to combine that principle with this other principle, that in so compounding them we must take due account of their respective phases, in order to account for the whole of the phenomena of diffraction, curious and complicated as they are. In other words, we have only to combine Huygens's principle with the principle of interference. These two principles again are nothing more than special applications of the general dynamical principle of the superposition of small motions; a principle which lies at the very basis of the theory of undulations, and of which the special applications just mentioned might have been foreseen.

The application of this principle to special cases, among others to the case of the external fringes, involves calculations of considerable complexity. Fresnel executed these calculations for the external fringes, and also made a series of most careful and accurate measurements of the positions of the fringes referred to the geometrical shadow under a variety of

circumstances. The theoretical distances of the several fringes from the geometrical shadow were a matter of pure prediction; for the only unknown quantity involved in the theoretical expression, the length of a wave, had been determined by Fresnel by independent methods, some of them, as for example that depending on the measurement of the fringes produced by interference mirrors, not involving diffraction at all, so that not a single arbitrary constant was left, to be determined by some one measurement of a fringe in some one particular case, whereby an at least partial accordance between theory and observation might have been brought about. On the other hand the distances of the fringes from the geometrical shadow in a variety of cases were most carefully measured micrometrically, and the comparison of the calculated and observed places manifested a truly wonderful accordance, the average error being only about the $\frac{1}{2000}$th part of an inch.

The distances calculated from the imperfect theory of Dr. Young agreed nearly, but not exactly, with those deduced from the complete theory of Fresnel: in spite of the smallness of the difference, the measurements were sufficient to discriminate between the two, and the result was decisively in favour of the complete theory as given by Fresnel.

As the geometrical shadow is not, like the fringes,

a visible object, it may not be superfluous to mention briefly the mode of referring the places of the fringes to the geometrical shadow. The fringes were formed by the cheeks of an aperture with parallel edges, one of which was moveable by a micrometer screw, by which means the breadth of the aperture could be very accurately measured. The cheeks were set a sufficient distance apart to prevent the fringes formed by the one affecting in any sensible way those formed by the other. We thence get from similar triangles, by the rule of three, the distance apart of the geometrical shadows at the focal plane of the eye-lens. Now the distance of any particular fringe, say the first dark fringe, on the right from the same fringe on the left can be measured by a micrometer moveable in the focal plane of the eye-lens. Half the excess of distance between the geometrical shadows in the focal plane over this gives the distance of the first minimum from the geometrical shadow.

The phenomena of diffraction may be varied indefinitely by varying the outline of the opaque body, or aperture or apertures pierced in an opaque screen, as well as the two distances concerned; and there is a large class of interesting appearances which may be seen by using a telescope with which a luminous point is viewed in focus, and covering the object-glass with a screen containing one or more apertures of any form

that may be chosen. In this case especially most curious and beautiful coloured patterns are produced, so strange and complicated that a person looking at them for the first time could never guess from the pattern what was the form of the aperture which produced it.

Besides the case of the internal fringes which was so carefully examined by Fresnel, a number of other instances in both classes, that is, without and with a lens or object-glass combined with the diffracting body, have been investigated theoretically, and the results compared with observation. The accordance is found to be absolutely complete even in the most minute particulars.

And now at last, as part and parcel of the complete theory of diffraction, we are able to explain the existence of rays; to show why it is that it is so nearly true that light proceeds in a straight course past bodies and through apertures.

The explanation may be given without entering into mathematical details. Suppose light coming from a luminous point which for simplicity's sake we may suppose to be at a practically infinite distance, an element of the sun's disk for example. Let it fall on a screen in which is a moderately small aperture, and consider the disturbance produced beyond the screen at a point, *P*, well outside the projection of the

aperture. Make *P* the centre of a set of concentric spheres with radii increasing by half the length of a wave. These will cut the plane of the aperture in a series of circular arcs, very close to one another in consequence of the extreme smallness of the wave length, and comprising between any two consecutive circles narrow slips of the aperture, such that two adjacent slips are very nearly equal in area. Now we have a right to regard each element of each slip as the source of an elementary disturbance, which reaches *P* after the lapse of a time proportional to its distance from *P*. Since corresponding points in consecutive slips differ by half a wave's length in their distance from *P*, and the elementary disturbances from them accordingly always reach *P* in opposite phases, so as to neutralize each other, we easily see that the total effect of one slip is very nearly indeed neutralized by that of its neighbour. This is still more nearly true if we take the disturbance produced by one slip and the mean of those produced by its two neighbours; and in this way, by taking each alternate slip and the mean of its two neighbours, each slip gets counted once, and once only, except at the two ends, where, however, the length of the slips dwindles away to nothing. Hence there is no sensible disturbance, and therefore no sensible light, for a point *P* situated as we have supposed.

If now we take a point P situated well inside the projection of the boundary of the aperture, it may be shown by similar reasoning that the disturbance, and therefore the illumination, is sensibly the same as if the screen in which the aperture is pierced were away.

This conclusion rests, it will be seen, on the extreme smallness of the length of a wave, in consequence of which an aperture, unless extremely small, is cut a great number of times by a series of concentric spheres with radii increasing by half a wave's length. There is no difference of explanation as regards light and as regards sound, save what depends on the difference of scale entailed by the difference of wave length. Take as regards light the case of a small circular hole say the tenth of an inch in diameter, and of distances from the luminous point to the screen in which the hole is pierced, and from that again to the screen on which the light is received, of say 8 feet 4 inches, or 100 inches, each. In this case, regarding the luminous patch on the screen as a whole, there would be no great diffusion of light, but the phenomena of diffraction would nevertheless be fairly pronounced. There ought to be a corresponding case of diffraction for sound; but on what scale? Take 50 inches as the length of a wave of sound, which would correspond to a musical note of moderate pitch. Taking as before the $\frac{1}{50000}$th

part of an inch as the wave length for light, the length of the wave of sound will be two and a half million times as great as the wave length of light. Consequently to obtain the corresponding case of diffraction for sound, our "small" circular hole would be obliged to have a diameter of rather more than 4 miles, say 4 miles, and the distances from the source of sound to the hole through which it passes, and from that again to the place where the sound is listened to, would have to be 4000 miles each.

It is remarkable that the existence of rays, which formed the great stumbling-block in the way of the early reception of the theory of undulations, is now shown to belong to a class of phenomena, those of diffraction, the complete and marvellously simple explanation of which afforded by the theory of undulations now forms one of the great strongholds of that theory.

Before leaving this subject I will briefly mention two or three instances of diffraction which from their paradoxical character or their importance are deserving of notice.

Reverting to the case in which light from a luminous point passes through a circular aperture, and is received on a screen beyond, consider the brightness at a point just in the axis on the receiving screen. If the relation between the two distances already so

often mentioned and the diameter of the hole be such that the sum of the distances from the luminous point to the edge of the hole and from thence to the central point on the receiving screen exceeds the direct distance from the luminous point to the latter by just half a wave's length, theory shows that the illumination is actually four times as great as if the screen in which the hole is pierced were taken away altogether. Suppose now the hole be enlarged. We might have said at first sight, supposing we were ignorant of the theory, "Of course that must increase the illumination at the central point, or at any rate cannot diminish it." On the contrary it does diminish it; and if the hole be enlarged till its area is just double what it was in the first instance, the centre of the illuminated space on the screen is a black spot. This theoretical result is easily realized in experiment; only as the wave length varies from colour to colour, and the proper distance of the receiving screen varies with the wave length, when the screen is in adjustment for the brightest part of the spectrum it is not quite in adjustment for the fainter ends, so that the spot instead of being perfectly black is faintly purple.

Again, suppose a circular disk is exposed to radiation from a luminous point, and the shadow is received on a screen at some distance, or rather viewed directly through an eye-lens. According to

theory, the very centre of the shadow will be a bright point, as bright as if the disk were away. This strange result, again, can easily be verified experimentally, easily at least if we are not too ambitious as to the size of the disk; for the delicacy of the experiment increases with the size of the disk, and at the same time the total quantity of light that we have to work with decreases. In repeating the experiment I have seen without difficulty the central spot, with the system of rings round it well formed, in the centre of the shadow of a disk of about the size of a sovereign.

Among the class of diffraction phenomena in which a luminous point or line is viewed in focus through a telescope, and a screen with one or more apertures is placed in front of the object-glass, there is one case of very special interest. It is that in which a line of light is used, suppose an extremely narrow slit through which the sun's light is reflected horizontally, and a fine carefully ruled grating is placed opposite to the object-glass, the lines of the grating being parallel to the slit. The best results are obtained with gratings consisting of glass, or sometimes metal, on which fine parallel lines are ruled with a diamond point. It is requisite that the lines should be very accurately equidistant, and in fine gratings they are so close that several thousand

go to an inch. If the grating thus constructed be of metal, it can only of course be used for reflection.

Now on viewing through the telescope the light transmitted through or reflected from such a grating, a most remarkable appearance is presented. The luminous line is seen through or by reflection from the grating as if the ruled lines were away, and right and left of it for some way the field is dark. But then on both sides we get *pure* spectra, the blue ends being nearest to the axis. These are followed by a second set of spectra, the blue ends of which overlap the red ends of the former, and so on, the spectra as we proceed overlapping one another more and more. So pure are these spectra if the grating be a good one, that they show the fixed lines of Fraunhofer to perfection.

The formation of these pure spectra can easily be explained, and the formula expressing the deviation for light of any one kind in the spectrum of any order in terms of the interval of the grating, the angle of incidence, and the wave length for that kind of light obtained, from the general principles of the theory of undulations. But by mounting the grating in the axis of a horizontal graduated circle which carries the telescope, the deviations can be measured with extreme precision. The interval of the grating

is got by measuring the breadth of the ruled space, and dividing by the number of ruled lines less one. We thus have the means of determining the wave lengths, if the theory be true, for as many definite kinds of light marked by definite lines as we please; and by comparing the wave lengths measured by means of spectra of different orders, at different inclinations of the incident light to the grating, and by different gratings, we have a very sharp test of the truth of the formula deduced from theory. The accordance is complete; and that being so we are justified in the interpretation assigned to that measurable quantity which we call a wave's length, and obtain its value with great precision.

I may here perhaps mention that it has even been proposed to take the length of a wave of light of some particular kind, such for example as that belonging to one component of the double yellow line of a soda flame, as a natural standard to which a national standard of length might be referred in case of loss. The French refer their metre to the dimensions of the earth. The English refer their yard to the length of the seconds' pendulum. But supposing the earth to be slowly contracting by cooling, both these natural standards would be liable to be affected in the course of ages; and if such a catastrophe were to occur as the impact on the earth of some great

globe visiting our solar system, the dimensions of the earth and the value of gravity, and accordingly the length of the seconds' pendulum, would at once be affected to an unknown degree. But the wave length of light of a given kind would remain unchanged, and the survivors of such a catastrophe might have recourse to it to recover the ancient standard of length.

LECTURE III.

Closer examination of the fundamental suppositions of the Theory of Undulations—Survey of the conclusions arrived at by a study of the phenomena of common light—Elementary facts of double refraction and polarization.

IT has been my aim in these lectures to endeavour to give you some definite idea of the evidence on the strength of which we assert that light consists in undulations propagated in a medium filling the interstellar spaces. To enable you to judge more fairly of the evidence, I have attempted to present the subject in an inductive rather than in a deductive form. Instead of starting with a number of hypotheses, originating you would not know how, and then showing how the conclusions following from them are in accordance with observed results, I have commenced with only the most fundamental conceptions of undulations, and of the conditions which we must suppose to obtain in order that they may exist, and have afterwards supplemented our original rather crude conceptions in the manner which a study of the phenomena showed to

be necessary. We have seen that not merely are the laws of reflection and refraction in agreement with the theory, but the curious and complicated phenomena of interference and diffraction are explained by it to the minutest particular. What strikes one most about the theory is what has been truly described as its *astounding simplicity*. This so carries on the face of it the stamp of truth, that to one who is familiar with the phenomena as well as the theory, an overwhelming conviction is produced that it is indeed true to nature. That being the case, it may be well now to examine the various hypotheses which must be made in some greater detail than has hitherto been done.

The fundamental hypothesis of the existence of a medium to which we give the name of ether, I have already mentioned. To account for undulations in this medium, we must attach to it the two radical conceptions of inertia and elasticity. First, a finite time must be required in order to generate in a finite portion of it a finite velocity by the action of a finite force. Secondly, a change of condition of some kind in the ether consequent on a relative displacement of its parts must call into play a force of restitution tending to restore it to its primitive condition. Thus in air condensations and rarefactions produce respectively an increase and a diminution of pressure;

so that any small portion of the air which has been contracted or expanded tends to push out the surrounding parts, or to be compressed by them, and so to return to its primitive state; and it is to the forces of restitution thus called into play that the propagation of sound is due. Naturally therefore those who adopted the undulatory theory of light were led to imagine the ether as possessing a similar kind of elasticity. There is however a whole class of phenomena which I have not yet so much as named, and which have no counterpart in sound, the study of which has led us to conclude that the elasticity of the ether is of an altogether different nature from that of air. The question of the relation between the direction of vibration of the particles of ether and the direction of propagation of the light is bound up with that of the character of the elasticity by virtue of which the undulations are propagated. In the explanation, however, of the ordinary phenomena of interference and diffraction, we are not concerned with the direction of vibration; in interference for instance, where we always have two streams of light from the same source pursuing nearly the same paths, and coming together either in the same direction, or in directions very slightly inclined to each other, whatever be the direction of vibration in one of the interfering streams, the same, or very nearly indeed

the same, will be the direction in the other, and that is all that we are concerned with in the explanation. And the explanation of the ordinary phenomena of diffraction has, as we have seen, been resolved into the interference, not of two, but of an infinite number of infinitely small disturbances all coming originally from the same source, and following very nearly the same paths.

The mode of excitement of the undulations in the first instance is in some respects analogous to the mode of excitement of sound by a vibrating body, such as a bell, but in some respects distinctly different. A bell in exciting vibrations in the surrounding air acts as a continuous elastic body; in explaining the mechanical action, we have nothing to do with speculations as to the molecular constitution of ponderable matter. But the fact that the spectra of flames show bright lines depending on the nature of the chemical substances in the flame, shows that in the excitement of the ethereal vibrations we have in many cases, if not in all, to do with the vibrations of the constituent parts of the chemical molecules of which we have reason to believe that ponderable matter consists.

The explanation of refraction on the theory of undulations requires us to suppose that in refracting media, such as glass or water, the vibrations are

propagated more slowly than in what we call vacuum. In fact, theory shows that the sine of the angle of incidence must be to the sine of the angle of refraction in the ratio of the velocity of propagation in vacuo to the velocity of propagation in the medium. The question then arises, When light passes through water or air, what is the vibrating medium?

At first sight we might say, of course the water or the glass itself. But this supposition will not bear examination. We know too much of the elasticity of water and glass to allow us to explain the result in that way. Water is sometimes spoken of as incompressible, but we know that it yields slightly to a compressive force. The amount of compressibility can be measured by hydrostatic means, and from thence the velocity of propagation of sound in water can be determined by calculation. It comes out about four times as great as that of sound in air, which agrees very well with the experimental measurement of the velocity which has been made in the Lake of Geneva.

Glass, and isotropic elastic solids in general, possess two distinct kinds of elasticity, one, by which they resist compression of volume, the other, by which they resist a distortion not involving alteration of volume, but opposed by the force by which the substance resists the gliding of one part over another,

a gliding which takes place freely in liquids. The two elastic constants of glass have been carefully determined, and from them can be calculated the two velocities of propagation of two kinds of disturbance which a large mass of glass would be capable of transmitting. These come out greater no doubt than, but still comparable with, the velocity of propagation of sound in air, and are therefore almost immeasurably smaller than the velocity required to account for the refraction of light on the supposition that it is the glass itself that vibrates. In fact, the velocity of light in vacuum is nearly 1,000,000 times as great as that of sound in air, and the velocity of light in glass would be equal to its velocity in air divided by the index of refraction of glass, or say about 600,000 times that of sound in air, so that it is altogether of a different order of magnitude.

Again, consider a gas or mixture of gases, such as air, in its relation to Light. Air can be rarefied till we have a near approach to an interstellar vacuum, or again can be condensed till its density becomes comparable with that of a liquid. Yet all through these changes there is a perfectly continuous change in its relations to light. It will not do therefore to say that light is propagated through air in one way, by one sort of mechanism, when the air is very rare, and by another when the air is very dense. But

when air is rare, and makes a near approach to what we call vacuum, but which we must now conceive as space filled with the luminiferous ether, it is clear that it must be by the vibration of the ether that light is propagated in it. We are led therefore to conclude, from these considerations again, that when light is propagated in a gas condensed till its density becomes comparable with that of a liquid, and accordingly in liquids also, and in solids like glass, which behave with respect to light just as liquids, it is ether existing in the interstices between the molecules of the bodies, of which the vibrations constitute the light that passes through them.

We might not perhaps have been disposed in the first instance to suppose that such a solid material as glass really had ether pervading it. But we must beware of applying to the mysterious ether the gross notions which we get from the study of ponderable matter. The ether is a substance, if substance it may be called, respecting the very existence of which our senses give us no direct information: it is only through the intellect, by studying the phenomena which nature presents to us, and finding with what admirable simplicity those of light are explained by the supposition of the existence of an ether, that we become convinced that there is such a thing. We know that a magnet attracts iron through a piece of

glass; and yet the magnetic influence is one which we can neither see nor feel: why then should not ether exist within glass, and be capable of vibrating within it?

It may readily be imagined, as more probable than the contrary, that the presence of the ponderable molecules interspersed through the ether, within the region of space which is enclosed by the surface of the glass, may have the effect of altering the velocity of propagation of the ethereal vibrations within it, and very probably diminish it. But what may be the precise mechanism by which this result is brought about we do not know. It is easy to frame plausible hypotheses which would account for the result, but it is quite another matter to establish a theory which will admit of, and which will sustain, cross-questioning in such a variety of ways that we become convinced of its truth.

It follows from Huygens's explanation of the law of refraction, which assumes nothing but what lies at the very foundation of the theory of undulations, that the ratio of the velocity of propagation of light in vacuo to its velocity in a medium, such as glass or water, must be equal to the index of refraction, and must therefore vary from one colour to another, increasing from the red to the violet, sometimes, as in the case of oil of cassia, as much as 6 per cent. and upwards within the limits of the visible spectrum. Now we know that in sound notes of different pitch are propa-

gated with the same velocity, as also follows from theory, and it has been suggested that it is a difficulty in the way of the theory of undulations that the case must be so different with light. I cannot say that it appears as such to my own mind; for the case of the vibration of one of two mutually penetrating media, as for example glass and the ether, is so different from any that we have to deal with in the case of sound that we cannot argue from the one phenomenon to the other. If indeed it were established that the velocity of light in a vacuum differs from one colour to another, we should then have to allow that an analogy which might have been expected to hold good between the two phenomena does not really do so. But till very lately the observed phenomena which reveal a finite velocity of light, and the experiments which had been made to determine the velocity directly, all yielded a negative result as to any difference of velocity between one colour and another, so that the difference in the ratio above-mentioned was attributable to a difference of velocity in the refracting medium. I have said "till lately," because in a paper by the late Dr James Young and Professor George Forbes, presented to the Royal Society between two and three years ago, and printed in the Philosophical Transactions, in which the authors have determined experimentally the velocity of light by a method founded on that of Fizeau,

but presenting certain new features, it is mentioned as a result of the observations that blue light seemed to travel faster in air than red light. The method employed is founded on the judgment of the observer as to the equality of intensity of two points of light, or artificial stars, seen simultaneously in the field of view. It is difficult to see any possible source of instrumental error which could have led to the above conclusion as to a variation of velocity depending on the colour: on the other hand, the conclusion rests on the judgment of the eye of a single observer; and till the question has been further tested it would seem to be premature to regard the difference as established. Should it be confirmed by further observation, it will then be necessary to consider whether the circumstance that the light was propagated in a field of magnetic force, namely the earth's magnetic force, may have had something to do with it. However even if the result of further enquiry should be to show that the different colours are propagated at different rates in a simple vacuum free from disturbing influences, all we should be entitled to say is that the expectation which we were led to form beforehand from the analogy of sound, and from the supposition that the forces whereby one portion of ether acts directly on another are insensible at finite distances, at distances even comparable with the $\frac{1}{50000}$th part of an inch, has not been verified.

As regards the mode of perception, while there are analogies between sound and light there are at the same time notable differences. In sound, the tympanum of the ear is thrown mechanically into vibration, and the nerves of hearing are mechanically affected, as a mechanical disturbance of a point on the surface of the body is made known by the sense of touch. But in light, just as we have seen reason to believe that it is the disturbance of the ultimate molecules, or of their constituent parts, by which the vibratory motion which constitutes light is in the first instance communicated from ponderable matter to the ether, so we have reason to think that when light is absorbed what takes place is that the disturbance of the ether is communicated, not to portions of matter regarded as forming portions of a continuous elastic body, but to the ultimate molecules of which matter consists, or to their constituent parts. It may be that temporary chemical changes are thereby produced in the ultimate filaments of the nerves of the retina, in which case the sense of sight would be more analogous to the sense of taste than to that of touch.

Corresponding theoretically to this difference is the fact that in light we have absolutely nothing answering to the sensation of harmony in sound. When two musical notes have their times of vibration in some simple ratio, we have concord when they are

sounded together; thus the ratio of 2 to 3 gives a fifth, that of 4 to 5 a major third &c., whereas when two notes sounded together have their times of vibration in no simple ratio, we have discord. But when two kinds of light, each of definite refrangibility, and accordingly of definite periodic time, are mixed together, there is no pleasing or disagreeable sensation depending on whether the periodic times are in some simple proportion or not.

And now before passing on to a totally different branch of the subject, the study of which leads us to believe that the properties of this mysterious ether must be very different from what we should have imagined beforehand, it may be well to pause and contemplate for a little the wonders with which our study of the phenomena up to the present point has shown that we are surrounded.

First, we learn to regard the interplanetary and interstellar spaces as no mere void, or empty space passed through by swift messengers in the shape of particles of light conveying information from distant worlds, but as filled with an ever present, all pervading substance, in which the ultimate particles of ponderable matter, including those of our own bodies, are continually as it were bathed, and yet of which our senses give us no direct cognizance.

Secondly, that whatever other important offices

this ether may fulfil, this one at any rate belongs to it, that it forms the medium of visual communication between ourselves and our fellow creatures, between ourselves and the various objects around us, between ourselves and distant worlds.

Thirdly, that this communication is carried on by tremors of some kind propagated through the ether with a velocity so enormous that for all practical purposes of communication on earth it may be deemed instantaneous. In fact, light would travel about seven and a half times round the whole earth in one second. But so rapid are these tremors that many hundreds of millions of millions take place in one second. Notwithstanding therefore the enormous rate of propagation, the lengths of the waves are excessively small, ranging about the $\frac{1}{50000}$th part of an inch.

It has been shown that it is this excessive smallness of wave length which enables light to be propagated so nearly in a straight course, in independent rays. Were it not for this, the formation of sharp images would be impossible. Were the lengths of the waves of light comparable with the lengths of the waves of sound, we should as regards the use of our eyes be nearly in the condition of a man who was all but blind; who could just distinguish light from darkness, or a gleam of red from a gleam of green, and no more. We should be in this condition if the

time of a vibration were anything like so great as the one hundred millionth part of a second, in which case the length of a wave of light would be comparable with that of a wave of sound.

Fourthly, we learn that notwithstanding the almost inconceivable shortness of the time of vibration, a variation in this periodic time is nevertheless recognisable by our senses, and that it is to this cause it is due that the face of nature does not present to us simply light and shade, like a photograph, but that we have that endless variety of colour which we enjoy.

Fifthly, in the plan of an elastic medium conveying small vibrations, we have a mechanism of the simplest possible kind having for result that rays of light from objects all around cross each other's paths in all sorts of ways without any mutual disturbance.

When we survey a varied landscape, each visible point in it, however minute, may be regarded as an independent source of light, from which the light proceeds in all directions. True, the objects are not in general self-luminous; they are seen by the light of the sun or of the clouds which they irregularly reflect, but as regards the behaviour of the pencils which proceed from them they are as good as self-luminous. Well then; from each visible point, however minute, there enters the eye every second a length of light of

about 186,000 miles, that is, light which would have travelled that distance had not the eye been there to catch it, this immense length being filled with undulations of lengths ranging about 50,000 to the inch. And if the landscape be contemplated by a multitude of persons, from each visible point in it that vast length of light, consisting of undulations of such excessive minuteness, enters the eye of each spectator every second of time; and all these various streams of light, proceeding in all sorts of directions, cross each other's paths in all sorts of ways without the slightest mutual disturbance.

To one previously unacquainted with the subject, these statements seem like the dreams of an enthusiast, or at best the speculations of some wild theorist, and yet there is nothing in what I have stated beyond the sober conclusions of scientific investigation, conclusions supported by an amount of evidence altogether overwhelming. In saying this it is to be remembered that the precise mode of disturbance of the ether has been left an open question.

In studying this subject, one can hardly fail to be struck with the combination of these two things:—the importance of the ends, the simplicity of the means. When I say the importance of the ends, I use a form of expression which is commonly employed as expressing design. And yet on that very account

we must be on our guard against too narrow a view. When we consider the subject of vision in its entirety, the construction of the recipient organ as well as the properties of the external agent which affects it, the evidence of design is such, it seems to me, as must to most minds be irresistible. Yet if I may judge of other men's minds by my own, it is rather in the construction of the recipient organ than in the properties of the agent that affects it, that the evidence of design is so strongly perceived. And the reason of this may be that we are here dealing with what more nearly resembles design as we know it in ourselves. Man takes the laws of matter as he finds them; the laws of cohesion, of the conversion of liquid into vapour, of the elasticity of gases and vapours, and so forth; and in subserviency to those laws he constructs a machine, a steam-engine for instance, or whatever it may be; but over the laws themselves he has absolutely no control. Now when we contemplate the structure of the eye, we think of it as an organ performing its functions in subserviency to laws definitely laid down, relating to the agent that acts upon it, laws which are not to be interfered with. We can it is true go but a little way towards explaining how it is that through the intervention of the eye the external agent acts upon the mind. Still, there are *some* steps of the process which we *are* able

to follow, and these are sufficient to impress us strongly with the idea of design. The eye is a highly specialized organ, admirably adapted for the important function which it fulfils, but, so far as we can see, of no other use; and this very specialization tends to make the evidence of design simpler and more apparent. But when we come to the properties of the external agent which affects the eye, we begin to get out of our depth. These more nearly resemble those ultimate laws of matter over which man has no control; and to say that they were designed for certain important objects which we perceive to be accomplished in subserviency to them, seems to savour of presumption. It is but a limited insight that we can get into the system of nature; and to take the very case of the luminiferous ether, while as its name implies it is all important as regards vision, the present state of science enables us to say that it serves for one object of still more vital importance; we seem to touch upon another; and there may be others again of which we have no idea.

In the study of those phenomena of light which I have hitherto brought before you, we derived constant assistance from our knowledge of the theory of sound. I now come to a branch of the subject where the theory of sound fails us altogether, to a class of

optical phenomena which have nothing answering to them in sound. I refer to double refraction and polarization.

Of these phenomena, the former of which is so closely related to the latter, double refraction was the first to be discovered. It was in 1669 that Bartholinus published his account of the discovery of a strange and unusual refraction in Iceland spar. The subject was taken up and investigated with the keenest interest by Huygens, whom we must regard as the founder of the theory of undulations. It would not be in accordance with the object which I have had in view in these lectures to enter into details respecting the phenomenon, and I must content myself with mentioning a few of the more salient features.

Iceland spar stands we may say alone among minerals in at the same time possessing powerful double refraction and occurring in large clear crystalline masses. It was this circumstance which led to the discovery. The mineral cleaves very readily in three definite directions, so that a block obtained by cleavage is of the form of a parallelepiped. The three obtuse dihedral angles of this parallelepiped are all exactly equal, and are so turned that two opposite solid angles are contained by three equal obtuse plane angles, while each of the remaining six is contained by one obtuse and two acute. A *direction*

—not, observe, any special line—equally inclined to the three edges which meet in one of the obtuse solid angles is called the *axis* of the crystal. The crystalline structure, so far at least as it is revealed by the cleavage planes, or I may add by the natural faces, is symmetrical with respect to three planes and no more, each of these being parallel to one of the edges which meet in an obtuse solid angle, and perpendicular to the plane of the other two, and accordingly being parallel to the axis of the crystal. Any one of these planes is what Huygens called a principal plane.

If an object near at hand be viewed through such a block, as for example if the block be laid on a printed page, two images of the object are seen, of which one appears more elevated than the other when both eyes are used, indicating a stronger refraction. This image is called the ordinary, because it obeys the ordinary law of refraction. That such is the case was found by Bartholinus, and confirmed by Huygens; and the accuracy of the ordinary law as applicable to this pencil had stood the test of the most refined measurements carried out by the most improved modern methods, among which I may specially mention the measurements made by Professor Swan and Mr Glazebrook. But the rays belonging to the other or *extraordinary*, as it is called,

image must obey some totally different law. If the eye and object be in the principal plane of the block, the extraordinary ray obeys the laws of ordinary refraction so far as this, that the refracted ray lies in the plane of incidence; but whereas in ordinary media there is no refraction at a perpendicular incidence, and at oblique incidences there *is* refraction, which goes on increasing as the angle of incidence increases, in the case we are now considering there *is* refraction at a perpendicular incidence, and at one particular oblique incidence a ray passes through *without* refraction. And if the block be turned round in the plane of its surface, so that the plane through the eye and the object, which I suppose perpendicular to the plane of the surface, is no longer a principal plane of the crystal, the refracted extraordinary ray does not so much as lie in the plane of incidence.

If a distant object be viewed through the block, there is no duplication, so that whether a ray passes through the block as ordinary or as extraordinary, the emergent ray to which it gives rise is parallel to the incident, just as in the case of ordinary refraction. This is true whatever be the inclination of the incident ray to the surface, and whatever be the plane of incidence. It depends however on the parallelism of the surfaces of incidence and emergence; and if these surfaces be inclined to each other, forming a prism,

then the duplication of a distant object is at once perceived. If a slit of light be viewed through such a prism, two spectra are seen, which are unequally deviated, and show in general unequal dispersion.

Now what notion are we to form of the cause of double refraction if we adopt the theory of undulations? According to the fundamental explanation of refraction given by Huygens, as there are two refracted rays, there must be two disturbances propagated within the crystal as the result of an elementary disturbance excited at a point of its surface, and these two must travel with different velocities. They will spread out from the centre of disturbance in two closed surfaces respectively, or it may be a surface with two sheets. For one of these, the velocity of propagation will be the same in all directions, and the surface will therefore be a sphere. This follows from the fact that the ordinary ray obeys the ordinary law of refraction. But as the other obeys some more complicated law, the surface for it must be other than a sphere, and the velocity of propagation must be different in different directions.

It has been already remarked that there are three planes of crystalline symmetry, and these, as might have been expected from the intimate relation of the optical properties to the crystalline structure, are also planes of optical symmetry; that is to say, all the

optical properties are symmetrical with respect to each of them. But there are other planes which are planes of optical, though not of crystalline, symmetry. Thus if a plate be cut perpendicular to the axis, the optical properties in it are symmetrical with respect to any plane through the axis; and if a plate be cut parallel to the axis, the plane perpendicular to the axis is a plane of optical symmetry as well as the plane passing through the axis, though the former cannot be, and the second is not in general, a plane of crystalline symmetry. In short, all the optical properties are symmetrical about the axis, the two poles of which are alike, so that the optical properties present the same degree of symmetry as an ellipsoid of revolution*. Accordingly the wave surface relating to the extraordinary ray must have thus much symmetry; and Huygens assumed for trial that it was a spheroid of revolution. As far as he could make out, the refraction in the direction of the axis appeared to be the same for the two rays, and he accordingly supposed that the sphere belonging to the ordinary ray, and the spheroid which he assumed

* According to Sir David Brewster, this is not altogether the case so far as relates to the properties of the light reflected from an artificial surface of the crystal. His observations appear never to have been published in detail, nor has anyone else, so far as I know, taken up the subject.

as the form for the extraordinary, touched one another in the axis. This equality of refraction along the axis we now know to be rigorously exact. The form of the wave surface being assumed, the refraction of the extraordinary ray followed at once from Huygens's construction; and the mode and amount of refraction were found to agree with the construction as near as the most accurate measurements made by Huygens could decide. Mr Glazebrook has recently executed a series of measurements of the refraction of the extraordinary ray in Iceland spar with all the exactitude of modern methods, guided by our increased knowledge of the subject; and the result is that no certain error of Huygens's construction can be detected.

So far however the laws of the extraordinary refraction in Iceland spar are merely empirical, based upon a happy guess as to the form of the extraordinary wave surface; it remains to be explained, if explain it we can, why there should be these two kinds of disturbance at all within the crystal, and why the form of the wave surface should be what we find it must be if the most fundamental principles of the wave theory are true, by which the form of the surface is connected with the observed refraction.

Huygens imagined that the ordinary ray was propagated by the vibration of the ether within the

crystal, while in the extraordinary the molecules of the crystal took part as well as the ether. I have already mentioned some of the strong objections which exist to the supposition that the propagation of light in media such as water or glass takes place by vibrations of the ponderable matter; and similar objections in good measure apply to the supposition that both the molecules and the ether take part in it. But be that as it may, Huygens himself, when he had nearly concluded his researches, discovered a remarkable phenomenon which ill accords with the supposition of his which I have just mentioned.

Suppose a second block of Iceland spar to be placed on top of the first, so that the two are in the same relative position which they would occupy if they formed one larger block. Then neither of the images, ordinary or extraordinary, seen through the first block is split into two in passing through the second block, but the ordinary of the first furnishes an ordinary in the second, and nothing more, and similarly the extraordinary an extraordinary. The same is still the case if the second block be separated from the first, or even inclined by turning in the principal plane. But reverting for simplicity's sake to the first relative position of the blocks, suppose that the second is turned round the common normal to the two adjacent surfaces. The moment the second block is

turned from the primitive position, each of the images which the first block furnishes is split into two by the second block. The relative positions of the two images forming the pair into which either of the original images is split is just the same as if that original image had been formed by common light, but the intensities are different. When the second block has been turned through only a small angle, the ordinary of the first furnishes mainly an ordinary in the second, but also a faint extraordinary, and similarly the extraordinary of the first furnishes mainly an extraordinary in the second, but also a faint ordinary. As the turning goes on, the faint images get brighter, and the bright images get weaker, till after turning through 45° the four are alike. As the rotation continues, the pair of images that had been the weaker get the brighter, and the pair that had been the brighter get the fainter, till after turning through 90° the pair of images that had at first appropriated the whole of the light disappear altogether, and the pair that began to spring into existence on first turning now alone are seen. In this position, that is to say when the principal planes of the two blocks are perpendicular to each other, the image formed by light which suffered ordinary refraction in the first block furnishes nothing but an extraordinary in the second, and the image formed by

light which suffered extraordinary refraction in the first furnishes nothing but an ordinary in the second. As the rotation goes on, the same series of changes are repeated, so that in a complete revolution the two pairs of images vanish alternately at every quarter of a revolution.

On account of the fundamental importance of this phenomenon, I must crave your indulgence for dwelling on it at some length, even though from its elementary character it must be familiar to those of you who have paid any attention to this branch of the subject.

Suppose that light coming directly from a luminous source, such as the flame of a lamp, is received on a screen with a circular hole. The screen will isolate a beam of light which, as I shall have occasion to deal with it only at short distances from the hole, I will call cylindrical. Let this beam be received, suppose perpendicularly, upon a block of Iceland spar bounded as usual by cleavage planes. In passing through the spar, it will be divided into two, an ordinary beam, which will pass straight on, and an extraordinary which will be deviated in a lateral direction in passing through the block, and will give rise to an emergent beam parallel to the incident, and accordingly parallel to the first emergent beam. If the hole in the screen be not too large to suit the thickness of the block, the two beams will

come out without overlapping, and may be examined apart. They are found to be of sensibly equal intensity whatever be the azimuth of the block around its normal. Making abstraction of the small quantity of light which goes elsewhere by reflection, we may say that the whole of the incident light is divided equally between the two beams.

Suppose now that we fix the block in any position, say with its principal plane vertical, and place a second screen with a circular hole to let pass the beam which went straight through the first block, while it stops the other. On examining this beam by a second block, which is turned round, we find that it is divided into two of unequal intensity, which vanish alternately at every quarter of a revolution, the whole of the light passing into an ordinary beam in the second rhomb when its principal plane is vertical, and into an extraordinary when it is horizontal. Hence whereas a beam of common light is propagated in some definite direction, but possesses no relation to space in any other direction, so that there is no plane passing through it which we can distinguish, merely by the properties of the beam itself, from any other, the beam we are now considering, namely that which passed through the second screen, possesses properties with respect to directions in space transverse to its direction of propagation; and if we knew nothing of

its history, but it were merely presented to us for examination, that would not hinder us from recognising, by means suppose of a rhomb of Iceland spar, the peculiar properties which it possesses, nor from fixing by observation alone the direction of those two rectangular planes, vertical and horizontal in the case supposed, with respect to which its properties are symmetrical, and in either of which if the principal plane of the examining rhomb be placed, an ordinary or an extraordinary beam, as the case may be, is alone produced in it. Light possessing this property, however it may have acquired it, is said to be polarized, and the plane with respect to which its properties are the same as are those of the ordinary ray in Iceland spar with respect to the principal plane, is called the plane of polarization. Its azimuth may be determined experimentally as being that of the principal plane of an examining rhomb when so turned as to transmit only an ordinary beam.

To go back now to the first arrangement, namely, that of a beam isolated by a first screen falling on a rhomb of Iceland spar, and then on a second screen provided like the first with a suitable hole, let this second screen be so placed as to transmit the beam which passed through the rhomb by extraordinary refraction, and stop the other, and then let the beam be presented for observation. On examining it with a

second rhomb we should find that it possessed identically the same properties as the beam obtained by ordinary refraction in the first rhomb, save that the properties of this beam are related to the horizontal plane precisely as were those of the former beam to the vertical plane; and it is into the horizontal plane that the principal plane of the examining rhomb must be turned in order that nothing but an ordinary beam may be transmitted through it. Hence, according to our former definition, we must say that the extraordinary beam passing through Iceland spar is polarized in a plane perpendicular to the plane of incidence.

More than a century elapsed after Huygens's discovery of what we now call polarization before it was known that polarized Light could be obtained otherwise than by or as an accompaniment of double refraction. But in 1808 Malus made the very important discovery that when Light is reflected from glass at a certain angle, the reflected ray is wholly polarized; and since the properties of the reflected ray are the same with reference to the plane of incidence as are those of the ordinary ray in Iceland spar with reference to the principal plane of the crystal, we must in accordance with our definition say that the reflected light is polarized in the plane of incidence. The light reflected at other inclinations possesses all the properties of a mixture of common light with light

polarized in the plane of incidence, and may accordingly be said to be partially polarized in the plane of incidence. The transmitted light, whether the light be incident at the polarizing angle or the angle of incidence be arbitrary, is found to be partially polarized in a plane perpendicular to the plane of incidence.

Malus found that Light is thus polarized by reflection from transparent substances in general, from varnishes &c., but not from metals, the light reflected from which is found to be only partially polarized in the plane of incidence. The angle of incidence on transparent substances required for complete polarization Malus found to vary from one to another, though he did not discover according to what law, a law afterwards made out by Brewster, namely, that the polarizing angle is that for which the reflected and refracted rays are perpendicular to each other; or, as it may be otherwise expressed, the tangent of the polarizing angle is equal to the index of refraction.

Malus's important discovery of the polarization of Light by reflection proves that polarization, whatever it may be, is something that may exist altogether independently of double refraction, and must therefore be something intimately bound up with the nature of Light in itself. The intimate connexion of double

refraction with polarization shows that we cannot hope to explain the former of these phenomena unless we can obtain some insight into the nature of what constitutes polarization.

LECTURE IV.

Phenomena presented on interposing a crystalline block or thin plate in the path of Polarized Light which is subsequently analyzed—Laws of Interference of Polarized Light—Theory of Transverse Vibrations—Conclusion.

WE have seen that when a beam of polarized light is examined by a rhomb of Iceland spar, it is divided into two of in general unequal intensity passing through the spar. As before, make abstraction of the small quantity of light which goes elsewhere by reflection, and call the intensity of the incident beam unity, and let us consider in the first instance the intensity of the beam which passes through the examining spar as ordinary. Let A be the azimuth of the principal plane of the examining rhomb referred to the plane of primitive polarization. Then the intensity, the relation of which to the angle A is the object of our search, must be such that it is equal to unity when A is nothing, decreases to nothing as A increases to $90°$, increases to unity again as A increases to $180°$, decreases again to nothing as A increases to

270°, and finally increases to unity as at first as A increases to 360°, having furthermore the same value for A negative as for A positive, and for $90^\circ - A$ negative as for $90^\circ - A$ positive. A very simple function possessing this property is $\cos^2 A$. If this be the intensity of the ordinary, since the rest of the light passes into the extraordinary, the intensity of the latter must be $1 - \cos^2 A$, or $\sin^2 A$, or the squared cosine of the angle between *its* plane of polarization and the plane of primitive polarization. Such was the law assumed by Malus, and called after his name. It has been verified by photometric measurements, and is of great importance with reference to the theory of what it is which constitutes polarization.

Suppose that light polarized in any way is subsequently analyzed, as it is called, whether by a thick block of Iceland spar furnished with screens so as to stop out one of the transmitted pencils, or by any of the more convenient methods more commonly employed. Let the analyzer be turned till the field of view is dark, the light falling upon the analyzer in that position being stifled, as in the case of a good tourmaline, or else sent elsewhere. If a block of Iceland spar be interposed between the polarizer and the analyzer, and turned round in its own plane, in general there is more or less restoration of light, there being only four azimuths of the block, separated by 90°, in a

complete turn in which the field remains dark as before the interposition of the block.

This restoration of light is very easily explained as a consequence of what has already been mentioned. The polarized light falling on the block is divided into two beams polarized respectively in and perpendicularly to its principal plane, which traverse the crystal independently though overlapping, and which emerge blended together. Each of these on entering the analyzer is again divided into two, polarized in rectangular planes, which are those of the plane of primitive polarization and the perpendicular plane, of which the latter alone is retained, and the two retained portions of each of the streams enter the eye together, and their illuminations are added together. A very simple application of Malus's law shows that if we take the intensity of the primitively polarized light for unity, and disregard the small loss by reflection, the intensity of the light entering the eye will be half the square of the sine of double the azimuth of the block, measured from one of the vanishing positions. This restoration of light in the dark field forms a very sensitive and easily applied test of the possession of double refraction by the substance interposed.

But an observation made by Arago about 1811 opened out quite a new field of research, remarkable

alike for the beauty of the phenomena, for the light they throw upon the nature of polarization, and for the information they afford us respecting the inmost structure of bodies. Arago found that when the interposed crystalline plate was very thin, as may easily be obtained with mica or sulphate of lime, the restored light was not as before white, but showed the most gorgeous colours, varying with the thickness of the plate, its nature, and the direction in which and amount by which it is inclined, if inclined it be.

The more powerfully doubly refracting be the substance interposed, the thinner as a rule is the interposed plate required to be in order to show these colours. But with a given substance, such for example as Iceland spar, the amount of double refraction varies immensely with the direction. Thus in Iceland spar, one of the most powerfully doubly refracting substances known, we have seen that in the direction of the axis the two rays are refracted alike. Accordingly if a plate of Iceland spar, even a thick plate, be cut perpendicular to the axis, and be interposed perpendicularly to the incident light in the dark field, a splendid system of coloured rings is seen, which are interrupted by a dark cross. The arrangement of coloured curves is still more remarkable in the case of biaxal crystals, but the simpler case of a thin crystalline plate is

better adapted to our present purpose, since in the other case there are too many things crowded at once upon the attention.

Let us revert then to the case of a thin crystalline plate interposed in the dark field, being held, as I will suppose, perpendicularly to the incident light. Even with this restriction, it would take too long to mention all the phenomena exhibited, and would be wearisome; nor is it advisable to treat the subject in this way, for in fact they have all been brought under the dominion of theory, and are best studied in connexion with it, except in so far as may be necessary to establish the theory in the first instance.

I shall restrict myself therefore to mentioning a few leading features of the phenomenon, premising that in the case of a doubly refracting plate in general, as in that of a block of Iceland spar, there are two rectangular directions in which the beams independently transmitted are respectively polarized, and that if the incident light is polarized, and the plane of polarization coincides with either of those directions, the whole of the light entering the crystal passes into the beam which is polarized in the plane of primitive polarization. The two rectangular directions above mentioned have been named the *neutral axes* of the plate.

If the thin crystalline plate interposed be of uni-

form thickness, it is seen of a uniform colour; if the thickness vary irregularly, as in the case of a plate of selenite obtained by casual cleavage, the colours are arranged in patches, corresponding to the varying thickness. If the plate be turned in its own plane, there are four rectangular positions in which the field is left dark as at first, which are those in which one or other of the neutral axes lies in the plane of primitive polarization, and in which accordingly there is no bifurcation of the incident light on passing into the crystal.

If the analyzer be turned through 90°, so that the planes of polarization of the polarizer and analyzer are now coincident instead of perpendicular, and the field before the introduction of the crystal is at its maximum of brightness, on interposing the crystalline plate the colours now seen are complementary in character to the former. They vanish altogether when either neutral axis comes into the plane of primitive polarization, and are less vivid than in the dark field except when the neutral axes are at an azimuth of 45° from the vanishing positions.

If the planes of polarization and analyzation be set at an arbitrary angle, and the crystal be turned in its own plane, there are eight positions in a complete revolution in which the colours disappear, giving place to white light of the same intensity as when the

plate is away. Between these critical positions, the colours have the character of those of the dark and of the bright field alternately. The critical positions are those in which one of the neutral axes lies in the plane of polarization or analyzation. Hence—and this is to be specially noticed—for the production of the colours it is essential that the polarized light we start with should be divided into two pencils in passing through the crystal, and that each of these again should be divided into two by the analyzer, of which one portion is retained.

If the crystalline plate be ground into the form of a slender wedge, the colours are arranged in bands parallel to the edge of the wedge, the bands for any colour being equidistant, and the scale larger for the red than for the blue. If the analyzer be set to give the dark and the bright field in succession, the tints of the wedge agree with those of the reflected system of Newton's rings in the former case, and the transmitted system in the latter.

The equidistance of the bands for any particular colour shows that the *law* of the order of the tints, as depending on the thickness of the plate, is the very same as in the case of Newton's rings, the *magnitude* of the thickness merely being very much greater than in that case. Nor is this all. If we know the doubly refracting energy of any particular

substance, suppose sulphate of lime, we can calculate the retardation of phase of one relatively to the other of the two rectangularly polarized pencils which a thin plate of the substance can independently transmit, in terms of the thickness of the plate, to which that retardation is proportional. Now Dr Young showed that when this is done the thickness of plate by which any particular tint is produced is just what it ought to be *on the supposition* that the colour is due to the interference of the two rectangularly polarized pencils which traversed the crystal independently. After all this we can hardly help supposing that the colours must in some way be due to interference. But if so, why are they not seen with common light, just as Newton's rings; why should it be necessary that the light should in some way be polarized, and the polarized light should in some way be analyzed, and that the crystalline plate should be interposed *between* the polarizer and the analyzer in order that any colours at all should be seen?

If we *assume* that the colours of crystalline plates in polarized light are due to interference, the laws of the interference of polarized light may be deduced from the observation of those colours without any experimental difficulty. But if it still be regarded as in any way an open question whether those colours

are due to interference, it becomes important to investigate those laws by means of experiments free from any such doubt.

Accordingly the series of researches by which Arago and Fresnel determined in a direct manner the laws of interference of polarized light must be regarded as making an epoch in the progress of the study of this branch of the subject. These experiments were made on the fringes of interference with which we had already become familiar as exhibited by common light; such fringes as those produced by the interference of two streams of light from the same source, such as two virtual images of a luminous point, or of two streams from the same luminous point, which after passing through two parallel extremely narrow apertures near one another diverged and mixed together. We have seen what a triumphant explanation the theory of undulations affords of the phenomena of interference and diffraction in the case of common light; and if we obtain the same fringes, with the appearance of which we are so familiar, in working with polarized instead of common light, we cannot refuse to admit that they too are due to interference.

The study of the interference of two streams of light polarized in the same way presents no experimental difficulty whatsoever. It is merely necessary

to use polarized light instead of common light in any of the ordinary experiments of interference. On doing this the phenomena of interference are found to be absolutely the same with polarized as with common light. But to polarize two portions of light from the same source, and proceeding nearly along the same course, in two rectangular directions, and yet ensure a very near equality in the lengths of their paths, or rather equivalent paths in air, is a matter of very great nicety, so small is the difference of path that would suffice to prevent any exhibition of interference of the usual kind. Nevertheless by a series of ingeniously devised and carefully executed experiments Arago and Fresnel succeeded in establishing conclusively under what circumstances polarized light is, and under what it is not, capable of manifesting the usual phenomena of interference.

The result of this enquiry was summed up in five laws relating to the interference of polarized light, which were derived directly from observation. One of these has already been mentioned. Another is that when two streams of light from the same source are polarized in rectangular planes, they show no phenomena of interference, notwithstanding a near equality of paths. Another, that the mixed stream as in the last case may be analyzed without any phenomena of interference being thereby revealed.

Another that when two streams of light from the same source are polarized in rectangular planes, and afterwards analyzed, they *do* manifest the phenomena of interference *provided* the original source were one of polarized instead of common light. Lastly, in the phenomena of interference produced by rays which have experienced double refraction, the place of the fringes is not in all cases determined solely by the difference of equivalent paths; in certain cases it is necessary to alter the difference of paths by half an undulation. And the rule they gave for determining under what circumstances the half undulation should be added and under what circumstances not amounted to this:—when the planes of polarization and analyzation lie in the same quadrant made by the neutral axes of the crystalline plane, the character of the interference is determined simply by the difference of paths; but when they lie in adjacent quadrants, we must alter the difference of paths by half an undulation.

These five empirical laws embrace the necessity for a polarizer and for an analyzer in order that colours should be seen in a crystalline plate; and taken in conjunction with Malus's law, regarded at present merely as an empirical law, they enable us to calculate completely the colours of crystalline plates under all the varied conditions which may exist as

to thickness of the plate, doubly refracting energy of the substance of which it is formed, azimuth of the neutral axes relatively to the plane of primitive polarization, azimuth of the plane of analyzation; and for that we have no occasion to enter into any speculation at all as to what it is that constitutes polarization. Nay more; if we take the laws of double refraction as known empirically as the result of direct observation, we may even calculate completely the coloured rings and curves about the optic axis or axes of uniaxal or biaxal crystals, without entering into any speculation as to what the theoretical interpretation of polarization may be.

The question now arises, can we embrace the five laws of interference of polarized light, and Malus's law, in a theory which shall comprehend them all, and which shall be at least hopeful for the explanation of double refraction and of the polarization of light by reflection; though as these may depend on a knowledge of what is the actual state of things which we do not possess, we cannot demand of necessity that it *shall* lead to their explanation.

In applying Malus's law to the calculation of the colours of crystalline plates, we are led to contemplate an intensity which we may take as unity in the incident original polarized light as giving rise to intensities $\cos^2 A$ and $\sin^2 A$ belonging to light

polarized in a plane making an angle A with the plane of primitive polarization and in the perpendicular plane respectively, and these again as giving rise to polarized beams the intensities of which are obtained by a re-application of the very same law. Now in studying the interference of common light, we saw reason to conclude that for light of a given kind, that is, of a given refrangibility, the intensity is measured by the square of the coefficient of vibration, and consequently the coefficient of vibration by the square root of the intensity. It is true that I have not been able to lay before you the full evidence on which this conclusion is based, as it would have involved some considerations of too mathematical a nature to be suitable to the present lectures, so that I have been obliged to leave the result to be accepted in some measure on the strength of authority. Consequently we are led by pure observation, combined with so much of theory as belongs to the study of common light, to contemplate a beam of polarized light in which the coefficient of vibration may be taken as unity as being divided (as for example in passing through a crystalline lamina) into two polarized in rectangular planes, at azimuths of A and $90^\circ - A$ to the plane of primitive polarization, the coefficients of vibration belonging to which are expressed by $\cos A$ and $\sin A$. But this is

identically the law according to which forces, or displacements, or velocities in directions *perpendicular* to that of propagation, and in or equally inclined to the planes of polarization, would be resolved. We are inevitably driven to the contemplation of a *something* about polarized light which admits of composition and resolution according to the above simple law. This "something" can hardly be other than the vibrations themselves, and we are thus led to conclude that in polarized light the vibrations are rectilinear, but instead of being in the direction of propagation, as from the analogy of sound the vibrations of Light might naturally have been expected to be, are *transverse* to the direction of propagation.

When polarized light is obtained by ordinary refraction through Iceland spar, or by reflection from glass at the proper angle or incidence, everything is symmetrical with respect to the plane of polarization. We must suppose therefore that in polarized light the vibrations are symmetrical with respect to the plane of polarization. This leaves two alternatives open: they may either be in the plane of polarization or perpendicular to the plane of polarization. So far as the explanation of the laws of interference of polarized light is concerned, it is a matter of absolute indifference which alternative we adopt, and some undulationists have adopted the one and some the

other. The question can only be decided, if it can be decided at all, by introducing more or less of dynamical considerations, and that introduces more or less of speculation, since the dynamical nature of the mutual action of ponderable matter and ether is in great measure unknown to us. Perhaps the argument which introduces least speculation as to the dynamical nature of such actions is that derived from diffraction at a considerable angle; though it is true that even here we cannot produce that diffraction without the intervention of ponderable matter. The result in this case, as I have elsewhere shown, is decidedly in favour of the supposition that the vibrations are perpendicular to the plane of polarization, which is the alternative that was adopted by Fresnel, and ultimately by Cauchy, though at first he adopted the other; and it is the one for which, independently of diffraction, there is I think the most to be said. But as I have remarked the theory of transverse vibrations taken by itself does not involve the decision of this question.

If such be the nature of polarized light, what notion are we to form as to the nature of common light? Suppose light coming in some definite direction to fall on a screen with a hole, and to be received at the other side on a block of Iceland spar. Then if the hole be not too large, the two beams

which are produced within the spar will come out separated from one another, and will show their polarization in rectangular planes. This will still be true however the screen may be moved about, so that the light falls on different parts of the face of the block. Now let the screen be removed altogether. The light will still be decomposed into two beams within the spar, giving rise on emergence to two beams polarized in rectangular planes, but the beams will be broad, and will mix on emergence. But the mixture is identical with, or at any rate is undistinguishable from, common light.* Now the two disturbances in rectangular planes give rise by their composition to a disturbance which is in the fronts of the waves, but is in general elliptical, including the extreme cases of circular and rectilinear vibrations, but with the elements of the ellipse changing, as we have every reason to expect, irregularly in all sorts of ways a great number of times in a second. For though the vibrations may be sensibly regular for thousands or it may be myriads together (and the phenomena of interference show that they must have

* Abstraction is here made of the loss by reflection, which is not quite the same for the ordinary and extraordinary, the higher refraction of the former being accompanied by a slightly more copious reflection, so that in the transmitted light there is a slight theoretical preponderance of intensity in favour of the extraordinary. This is however so small as to be barely sensible in refined experiments, and for our present purpose it is best neglected.

a high degree or regularity) yet we should have no reason *a priori* to expect that they would remain regular for the fifty millions of millions or so of vibrations which must take place in the tenth of a second, the time for which an impression made on the retina is estimated to last.

Such being our notion of common light, the division of common light into two rectangularly polarized beams which follow different paths must be taken to imply that for some cause yet to be investigated the vibrations, which at first were in the fronts of the waves, but in other respects of any kind, are decomposed into two rectilinear vibrations in rectangular directions, which are propagated along different paths.

This fundamental conception as to the nature of polarized light, and its relation to common light, explains in the simplest manner the six laws relating to the interference of polarized light, and to the intensity of the polarized streams into which polarized light may be divided, to which I have just referred. The interference of light merely demands, so far as direction of vibration is concerned, that it should be as nearly as possible the same for the two interfering streams, a condition satisfied by two streams polarized in the same way. Again, the kinetic energy of a set of vibrations, to which for light of a given kind the intensity is theoretically proportional, is the sum of

the kinetic energies of the components in any two rectangular directions, irrespective of their difference of phase, and therefore no phenomena of interference ought to be visible in the mixture of two rectangularly polarized portions of light, even though they came originally from the same polarized source. Again, if common light be divided into two rectangularly polarized portions, which are afterwards subdivided in a similar manner, and a pair of these latter components which are polarized alike mix, having had but a small difference of path from the original source, they ought not to show any signs of interference. For as there is no fixed and permanent relation between the relative magnitudes or the relative starting-times of the first components, they are as good as if they came from two independent sources, in which case no phenomena of interference are either theoretically observable or experimentally observed, do what we will with the streams of light afterwards. But if the original source of light be one of polarized instead of common light, the case is altogether different. Then, whatever changes take place during the tenth of a second in the amplitude or starting-time of one of the first components, exactly the same take place in the other, and are carried on into the second components, of which therefore those which are polarized in the same way are in a condition

to interfere. And as to the circumstances under which in this kind of interference the difference of path must sometimes be imagined altered by half an undulation, the matter is simple to the last degree on the theory of transverse vibrations; it merely involves attention to the sign of a geometrical projection; and if we express the intensity of the mixed light, for the case in which the planes of polarization and analyzation lie in the same quadrant formed by the neutral axes of a crystalline plate, by a formula in which the symbols denote displacements or velocities or of an ethereal particle instead of intensities, the formula takes care of itself, and applies equally to the case in which the planes of polarization and analyzation lie in adjacent quadrants.

If polarized light be incident on a crystalline plate of uniform thickness, and the emergent light be viewed through an analyzer which is turned round, then except in certain special cases which I need not particularize, the light is not extinguished by the analyzer however it be turned, but merely becomes, in general, alternately a maximum and a minimum alternately at every quarter of a turn. So far it agrees exactly with partially polarized light. And yet the two are altogether different, and the difference may be seen at a glance by viewing them through a Nicol's prism capped by a plate of Iceland

spar cut perpendicular to the axis. The light we are now considering is called elliptically polarized light, and in contradistinction to it the light which I have hitherto called polarized has been denominated plane-polarized.

Elliptically polarized light may be obtained independently altogether of double refraction. The theory of transverse vibrations presents to the mind a very clear picture of what constitutes it. But as this is a matter of detail not involving any fresh principle, I forbear to enter further into it.

Being now armed with a definite theory as to the nature of polarized light, we are prepared to consider whether any explanation can be given of double refraction, as an accompaniment of which polarization was first discovered. Whether we are able or not to give a complete explanation of it, we might expect that the theory would at least so far fall in with it as to point hopefully towards an explanation.

The most salient feature of double refraction, interpreted by the aid of the theory which makes light to consist in undulations, and of that which specifies the nature of polarized light, is that when light falls on a crystalline plate or prism it gives rise to two kinds of disturbance within the crystal, which are propagated with different velocities, and in which the vibrations are rectilinear, and take place in planes

which are perpendicular to each other, or at least very approximately so, and of which the directions are determined by lines fixed in the crystal.

Now we have not far to go to find a mechanical illustration of such a mode of action. Imagine an elastic rod terminated at one end, and extending indefinitely in the other direction. Let the rod be rectangular in section, the sides of the rectangle being unequal, so that the rod is stiffer to resist flexure in one of its principal planes than the other. Let this rod be joined on to a cylindrical rod forming a continuation of it which extends indefinitely. Conceive the compound rod as capable of propagating small transverse disturbances in which the axis of the rod suffers flexure. Imagine a small disturbance, suppose periodic, to be travelling in the cylindrical rod towards the junction. It will travel on without change of type even though the flexure of the axis be not in one plane. But to find what disturbance it excites in the rectangular rod, we must resolve the disturbance in the cylindrical rod into its components in the principal planes of the rectangular rod, and consider them separately. Each will give rise in the rectangular rod to a disturbance in its own plane, but the two will travel along the rod with different velocities. This illustrates the subdivision of a beam of common light falling on a block of Iceland spar into two beams

polarized in rectangular planes, which are propagated in the spar with different velocities. Again, suppose the original disturbance in the cylindrical rod confined to one plane. If this be either of the principal planes of the rectangular rod, the more slowly or the more quickly travelling kind of disturbance, as the case may be, will alone be excited in the latter; and if the plane of the original disturbance be any other, the components into which we must resolve it in order to find the disturbance excited in the rectangular rod will in general be of unequal intensity, their squares varying with the azimuth of the plane of the original disturbance in accordance with Malus's law. This illustrates the subdivision of a beam of polarized light incident on Iceland spar into two of unequal intensity polarized in rectangular planes, and their alternate disappearance at every quarter of a turn. We see with what perfect simplicity the theory of transverse vibrations falls in with the elementary facts of polarization discovered by Huygens, standing in marked contrast in this respect with the conjecture by which Huygens himself attempted to account for double refraction.

But can we go further, and account for, or discover, from theory the laws of double refraction and the accompanying polarization in different directions in doubly refracting crystals?

It is to Fresnel we owe the first theoretical deduction of the laws of double refraction in Iceland spar, and the discovery of the beautiful laws of double refraction in biaxal crystals; laws some of which had been previously known from observation, while in other respects theory served to correct what had been supposed to be the result of observation, but which more careful observation carried out in directions indicated by theory proved to have been incorrect. The generalization by which Fresnel passed from the laws of double refraction in uniaxal to those in biaxal crystals is one of the most splendid things which has been done in science. And yet the theory which guided him to the discovery of these laws is not one which is rigorous throughout, nor did Fresnel himself profess that it was, though in some reproductions of his theory contained in text-books it is presented as if it were, to the detriment of the student. Fresnel was a man of singular sagacity, endowed apparently with a mind of the inductive class, leading him often to the discovery of truth from conflicting or imperfect evidence. We may even say it is fortunate that Fresnel did not rigorously follow out to their conclusions the premises with which he started, for had he done so he would have missed the discovery of the elegant laws of double refraction. In fact, Cauchy and Neumann independently worked out the con-

clusions which rigorously follow from the state of things assumed by Fresnel; but with all the squeezing which the arbitrary constants furnished by theory admit of, they were not able to obtain Fresnel's laws except as an approximation. Had this been all, it is possible that the more complicated laws expressed by their formulae might have fitted observation as well as the simpler and more elegant laws of Fresnel. But the theory is hampered by a third ray which cannot be got rid of, and which leads to conclusions at variance with observation. While admiring therefore the geometrical part of Fresnel's theory, we must reject the mechanical conditions which he supposed to exist in crystals, as not being in conformity with nature.

Fresnel's laws have been obtained in an extremely elegant manner, as the result of a rigorous theory, by Green, who starting with an assumed mechanical state inclusive of but more general than that assumed by Fresnel, and subsequently limiting the generality by a single condition which the phenomena of light would naturally lead us to introduce, arrived directly at Fresnel's laws. It was necessary however to suppose the vibrations of polarized light to be *in* the plane of polarization. In the same paper he showed however that by starting with a mechanical state still more general, limiting it as before, and introducing

two simple linear relations between the arbitrary constants remaining, Fresnel's laws were again obtained, but this time by supposing that in polarized light the vibrations are *perpendicular to* the plane of polarization. Almost simultaneously with Green MacCullagh obtained equations of motion of the ether in a crystal identical with those of Green in his first theory, though by a method which does not seem quite so satisfactory as that of Green. They led of course to the same laws, and required the same supposition as to the direction of the vibrations in polarized light. Lamé has given a theory substantially agreeing with Green's first theory; and last but not least we have the electro-magnetic theory of Maxwell, which without any straining or assumption of relations between constants leads directly to Fresnel's laws.

It may seem strange that we should arrive at the very same laws by such different theories; first by one which is not a rigorous theory at all, and then by others which are rigorous, but which differ among themselves, even to such an extent that in one the vibrations in polarized light must be assumed to be in, in another perpendicular to the plane of polarization. In explanation of this it is to be observed, first, that all these theories alike involve the idea of transverse vibrations, and secondly that Fresnel's laws

are really the simplest which can in any way suit the phenomena. Fresnel's laws are embraced in an elegant construction applied to an ellipsoid; and just as an ellipsoid is the simplest generalization of a sphere when we pass from what is alike in all directions to what varies from one direction to another, so Fresnel's laws are really the simplest that the nature of the phenomenon, viewed in the light of the theory of transverse vibrations, admits of. It is not therefore so wonderful as at first sight might appear that the same laws should be arrived at from theories so different; and while the deduction of these laws is a strong confirmation of the truth of the theory of transverse vibrations which is common to all the methods, it is not itself alone to be taken as establishing the truth of the supposition as to the mechanical state of things in crystals which has been made in the deduction of the laws.

I have mentioned Malus's discovery of the polarization of light by reflection, and the question may naturally arise, is this reconcileable with the theory of transverse vibrations? To show that it is, I need only refer to Fresnel's deduction of the intensity of the light reflected from an isotropic transparent medium, according as the vibrations are in or perpendicular to the plane of incidence. These formulae were obtained by Fresnel with his wonted sagacity

from a process only partially complete, since we must either allow that a part only of the necessary equations of condition at the common boundary of the media are satisfied, or else that the mechanical state for which the conditions employed give the complete solution remains to be defined.

Fresnel's deduction of his laws for the intensity of reflected light was made on the supposition that in polarized light the vibrations are perpendicular to the plane of polarization. A slight difference of hypothesis as to the state of things would lead, by reasoning very similar to that of Fresnel, to the very same two formulae only with the directions of vibration to which they respectively apply interchanged. On the present supposition therefore the vibrations in polarized light must be supposed to be in the plane of polarization.

The polarization of light through the unequal absorption belonging to the two rectangularly polarized pencils within certain coloured doubly refracting crystals, such as tourmaline, readily falls in with the theory of transverse vibrations. For there can be no doubt that absorption in general consists in the expenditure of the incident ethereal vibrations in producing molecular agitation; and it is easily understood that the molecules may be more easily agitated by an ethereal vibration in one direction than in another.

The aim which I have proposed to myself in this first course of the Burnett lectures has been to lay before you, as impartially as I could, a summary of the evidence on which we accept the answer to the question, What is Light? given when we say, Light consists of undulations in a medium, called ether, pervading the interplanetary and interstellar spaces, and existing also within bodies formed of ponderable matter. Difference of refrangibility, with the accompanying difference of colour, depends upon a difference in the frequency of these undulations. The direction of vibration of the particles of the ether is transverse to the direction of propagation of the light, and accordingly (at any rate in the case of vacuum or an isotropic medium) the vibrations take place in the fronts of the waves, but in common light are not otherwise restricted, while in polarized light they are rectilinear, taking place in a direction which is symmetrical with respect to the plane of polarization.

Naturally the full force of the evidence can be felt only by those who have well studied the subject. I hope however that I may have succeeded in showing, even to those who previously may have paid little attention to the matter, that at least there are powerful arguments in favour of the accepted answer. The inductive arrangement, which seemed best fitted for

the object I had in view, naturally led to a treatment in good measure historical, but I have not, I hope, neglected recent researches. I have endeavoured to discriminate, and to lead you to discriminate, between what is well established and what is still speculative, and have confined myself almost entirely to the former.

Should I be permitted to deliver courses of lectures again in the two following years, it is my intention, in accordance with a scheme communicated in outline to and approved by the Burnett Trustees, to devote next year's course to researches in which light has been used as a means of investigation, while the third year's course would be assigned to light considered in relation to its beneficial effects. While the different objects to be held in view in these lectures are more or less blended together, the second course would more especially relate to recent researches, while the third would naturally harmonize with the original intentions of the Founder of the Trust.

END OF FIRST COURSE.

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LECTURES ON LIGHT.

SECOND COURSE.

ON LIGHT AS A MEANS OF INVESTIGATION.

LECTURE I.

Subjects of the present course—Use of the mode of absorption of Light by substances as a discriminating character—Examples—Various effects produced by the action of Light incident on bodies—Phosphorescence—Epipolic dispersion of Light—Fluorescence—Its use as a means of discrimination—Phosphorescence produced by electric bombardment—Delicate test of Yttria thus afforded.

IN the course of lectures which I had the honour of delivering to you last year, I dwelt on the nature of light itself, and endeavoured to give you a fair idea of the evidence on which we accept that view of its nature which is now I may say almost universally held by scientific men, namely, that light consists in undulations propagated in a highly elastic

medium called the ether, and that those vibrations are not, as we should previously have imagined, to and fro in the direction of propagation, as we know are those of air in the propagation of sound, but transverse to the direction of propagation. At the conclusion of the course I stated that according to an arrangement adopted in consultation with the Burnett Trustees the subject of the present year's course should be investigations carried on by the aid of light: or in other words on light as a means of investigation.

In one sense of course this would include nearly every investigation that we can carry on; for in nearly every case we make use of our eyes, and without light they would be of no avail. But it is obvious that it cannot be in this comprehensive sense that the title of the present course is meant to be taken. The investigations actually intended are those in which the properties of light in their relation to ponderable matter enable us to ascertain something regarding the nature or the condition of that ponderable matter. Even as thus restricted the subject remains a wide one, branching out into other departments of science, especially chemistry, mineralogy and astronomy.

The special subjects belonging to our general class which I have selected to bring before you

are (1) absorption, and its application to the discrimination of bodies; (2) the emission of light consequent on absorption, or produced by different means other than incandescence, and its application as a test of the presence of particular bodies, or of the condition of the bodies so emitting it; (3) the rotation of the plane of polarization of polarized light, and its connexion with the constitution of bodies; (4) the emission of light by incandescent bodies in the state of vapour, and its application as a test of the presence of particular bodies; (5) the information thus afforded as to the constitution or condition of distant bodies; (6) the influence of the motion of bodies on the refrangibility of the light emitted, absorbed, or reflected by them, and the information thence afforded as to the motion of distant bodies.

That colours in all cases depend primarily on the heterogeneous nature of light, on its consisting of kinds differing from one another in refrangibility, and in the sensation of colour which they produce when separated one from the other, was long ago shown by Newton. In all cases the existence of colour depends either on the emission of a certain kind or certain kinds of light in excessive proportion in the source of light, as in the case of coloured

flames, or in a subsequent treatment of the light of such a nature that certain kinds are preserved for reception by the eye to the exclusion of others, or preserved in larger proportion than others. The production of colour by fluorescence, of which more presently, forms no exception to this rule provided we regard, as we may regard, the fluorescent body as the source of light, albeit in this case a secondary source.

By far the commonest exhibition of colour belongs to what is called absorption. This applies for instance to the verdure of the fields, the colours of flowers, those of dyed dresses, &c.

If we take for example a coloured flower, and hold it in different parts in succession of a pure spectrum, it appears of the colour of the part of the spectrum in which it is placed, but its luminosity varies greatly from one part of the spectrum to another, the flower appearing comparatively bright in those colours which approximately agree with that under which it is seen in white light, and comparatively dark, and sometimes almost black, in other parts of the spectrum. This shows, as Newton pointed out, that the proximate cause of the exhibition of colour in ordinary light is that the flower reflects copiously light of certain kinds, and reflects feebly or hardly at all light of other kinds.

But what is the nature of the selection of certain

colours for reflection while others are not reflected? Newton's attempt to explain this by reference to the colours of thin plates was not a happy one, and the inadequacy of such an explanation has long been recognised.

To illustrate the true answer let us take one of the commonest of all colours, the green of vegetation. If a green leaf be put into alcohol—a plant with an acid juice, like sorrel, should be avoided, as in that case special treatment is required to avoid decomposition—the green colouring matter is dissolved, and we obtain a beautiful clear green solution. [This was shown.] There cannot be any doubt that the cause of the colour in the leaf and in the solution is the same, but in the solution it is quite plain that it is in the *transmitted* light that the colour is seen. If we examine the behaviour of the liquid with respect to any particular kind of light, we find that as the light travels onwards in the solution it continually grows weaker and weaker, and presently becomes too weak to be any longer perceived. It is found that in passing across a layer of the liquid of given thickness a given fraction of the light is lost, from whence it readily follows that as the light travels on in the solution its intensity decreases in geometric progression as the distance traversed increases in arithmetic progression. But the rate of loss varies immensely from

one kind of light to another. For one kind there may be hardly any loss at all: the liquid may behave almost like water; while for another kind the absorption may be very rapid, and the liquid may behave almost like ink. For other kinds again the rate of absorption may be intermediate, and the liquid behave like water with some ink mixed with it.

Our clear solution requires to be looked through in order that the colour may be seen; for if it be looked at, and the colour seen by means of light which has been reflected from the further side of the vessel, that comes to the same thing.

Now suppose we mix some flour or powdered chalk with the liquid. We shall be able to see the colour by looking at the liquid mixture; [This was shown.] but that is evidently because the white powder reflects light from its surfaces in an irregular manner, and the light so reflected has had to pass through the solution in the interstices between the reflecting surfaces before it reaches the eye. It is just the same thing in a natural leaf; the irregularities of the structure cause irregular reflections and refractions of light in the substance of the leaf, and the light in its progress is exposed to absorption on part of the colouring matter.

The colour of solutions of metallic salts or of organic substances has naturally long been used as a

means of discrimination. But the eye can only very imperfectly judge of the composition of a mixed light by means of the colour. When the mixture is resolved into its constituents by means of a prism, it can then be seen at a glance how each constituent kind of light has been affected.

Now, in many cases some two or more parts of the spectrum are specially attacked by the coloured substance, and when that is the case the character and position of the bands of absorption when the light has been strained by passing through a suitable thickness of the coloured substance, which is usually most conveniently examined in the condition of a solution, or of a clear glass, are often highly distinctive. Till of late years, chemists were unaware of the simple means of discrimination thus afforded, and occasionally made mistakes which a single glance at the absorption spectrum of the substance they had under examination would have prevented. Physicists indeed were familiar with these phenomena, but they usually studied them with other objects in view. Now, in consequence of the labours of Bunsen and Kirchoff, a spectroscope of some kind is considered an indispensable portion of the equipment of a chemical laboratory.

I will mention two or three examples. Under certain circumstances red solutions are obtained by

the action of acids or acid salts on certain oxides of manganese, which resemble a good deal in colour the permanganates, and which like them are oxidising agents. It seemed not unnatural to infer that the colour of the former class of solutions was due to permanganic acid, and a paper was written in one of the scientific journals in support of this view. A single glance at the spectrum of the transmitted light would have shown this to be an error, for the permanganates are characterised by a remarkable and highly distinctive system of bands of absorption, of which there are five occupying the region of the brightest part of the spectrum. These are wholly wanting in the spectrum of the solutions of the class first mentioned.

A long time ago the eminent chemist M. Fremy attempted to divide chlorophyll, as the colouring matter of green leaves is called, into a blue and a yellow substance, of which it was a mixture. He attempted to carry out this idea in two ways. In one he added to the alcoholic solution hydrate of alumina and a very little water, and filtered. The filtrate was of a yellow colour, and the precipitate on being treated with appropriate solvents for the colouring matter it contained yielded solutions which were not indeed blue, but of a decidedly bluer green than before. In another attempt he dissolved

chlorophyll in a mixture of hydrochloric acid and ether, when on separation of the two solvents an upper yellow stratum was obtained and an under one which was nearly blue. Fremy designated the colouring matters contained in the yellow solutions obtained in these two ways by the same name phyxo-xanthine; whereas the prism shows that the first is a definite colouring matter characterised by two bands of absorption in the blue, but the yellow solution obtained by the ether process is a complex mixture.

In the detection and separation of the rare earths of the Ceria and Yttria groups, the number of which has recently been largely increased, and which chemists are now actively engaged in investigating, great assistance is derived from the very peculiar spectra which the solutions of the salts of some of them present. Dr Gladstone was I believe the first to point out the peculiar spectrum of the salts of didymium, which show bands of absorption almost rivalling those of coloured gases in their narrowness.

This brief sketch may serve to give some idea of the way in which the relations of light to ponderable matter may be turned to account in investigations belonging to a branch of science altogether different; though instances far more striking will fall under our notice in a future lecture. I pass on now to a different though allied subject.

In the phenomenon of absorption, as the very name indicates, light is as it were swallowed up and disappears. But, as belonging to the incident light, a certain amount of energy is continually being brought to bear on the body in which the light is absorbed, and this energy cannot be annihilated; there must be something to show for it.

Now different effects are produced in different cases, or it may be are simultaneously produced in the same instance. The commonest effect of all, one indeed that would seem to be always present, whether alone, or mixed with others, is that of raising the temperature of the body on which the light falls. It is true that the rays which are the most luminous are by no means those by which this effect is most powerfully produced; and that they are far surpassed in this respect by certain rays lying beyond the red, which though physically identical in their nature with rays of light, from which they differ only in the same way in which rays of one colour differ from those of another, do not nevertheless affect the eye. Still, the effect is produced by rays of whatever refrangibility, though feebly it is true by those of high refrangibility. But this effect of absorption, though the commonest of all, is not of a nature to be made available as a means of discrimination.

Another effect sometimes produced is that of effecting chemical changes. On this is founded the whole practice of photography. Although the seat of the chemical change varies from one part of the spectrum to another, with a variation in the chemical nature of the substance acted on, the variation cannot, except in rare cases, be conveniently used as a means of discrimination. Photography may indeed be most usefully employed for the purpose of detecting and recording what is going on in different parts of the spectrum, especially in the case of the ultra-violet and ultra-red regions, where eye observations fail. But this does not properly come under the subject of the present course.

Another effect sometimes produced as a result of the absorption of incident rays of light is that of phosphorescence or fluorescence. It has long been known that certain precious stones, especially the diamond, and the sulphides of the alcaline earths after exposure to light shine for some time in the dark.

[A collection of phosphorescent sulphides was shown, which glowed with different colours after having been excited by the radiation from burning magnesium wire.]

The laws of this phenomenon have been investigated by many physicists, especially by M. Edmond Becquerel, who has now given a collected account of his labours and those of his predecessors in

his work entitled *La Lumière*. This phenomenon forms no exception to the statement I have made, that in the phenomenon of absorption the incident light is swallowed up and disappears; for though it is true that as a result of that absorption light is forthcoming, yet that light is not in any way of the character of the incident light, but of a different composition altogether. The phosphorescent body is rendered for a time self-luminous as a result of the action of the light upon it, but the incident light itself is spent in the process.

This phosphorescence of long duration has not however been much used as a means of distinguishing one body from another, with one remarkable exception, in the case of a similar effect produced in a different way, to be mentioned presently.

There is however a phenomenon which appears to differ from phosphorescence only in degree, in point of duration, which is more generally available for this purpose, at least in the case of organic bodies.

In the course of some experiments on the action of light on vegetable colours, in which he had occasion to throw a more or less pure spectrum on the substance to be examined, Sir John Herschel noticed a remarkable prolongation of the spectrum beyond the violet, which is usually regarded as the termination of the visible spectrum, when the spectrum

was thrown on turmeric paper and a few other substances. This extension Sir John Herschel attributed to a peculiarity in the reflecting power of the paper: and as, in the case of turmeric paper, which showed the phenomenon best among the substances which he tried, the prolongation was coloured yellow, he even speculated on a possible repetition of the colours of the spectrum in order after passing beyond the violet.

Some years later he observed a curious phenomenon in a solution of sulphate of quinine of moderate strength. This liquid when seen by transmitted light appears colourless and transparent like water, but when viewed otherwise exhibits in certain aspects a peculiar blue colour. This, Herschel found, proceeded from a narrow stratum of the liquid adjacent to the surface by which the light entered, though a small part came from greater depths; and the blue light emanated from this stratum in all directions. But the most remarkable thing was that the light which had once produced this effect, though not apparently altered by transmission through the liquid, was deprived of the power of producing the effect; so that the light which had passed through a cell filled with the liquid, and then fell on another vessel containing a similar solution, no longer produced a blue stratum near the surface by which it entered the second solution. Herschel called the

phenomenon of the production of the blue stratum *epipolic dispersion*, and designated as *epipolised* the light which, by passage through such a solution, had been deprived of the power of producing the blue stratum again, though he did not explain wherein epipolised differed from common light.

I have already alluded to the careful study made by M. Edmond Becquerel of the phenomenon of phosphorescence. While engaged in this research he noticed that when a pure spectrum was allowed to fall on several of the phosphori examined (especially sulphides of the metals of the alcaline earths), in certain regions of the spectrum, whether belonging to the visible part or to the invisible region of greater refrangibility, the phosphorus shone with a light of a different kind from that which fell upon it, but only so long as the light remained falling upon it, differing in this respect from the ordinary phenomena of phosphorescence, in which the phosphorescent light lasts a very appreciable time. This phenomenon he rightly regarded as a phosphorescence of very brief duration; but from connecting it too closely with ordinary phosphorescence he failed to perceive its full bearing; and though he was actually working with an acid solution of quinine, the "dichroism" of which he expressly mentions, and had determined by photography its great opacity for rays of high refrangi-

bility, he never thought of putting these things together, or perceived their intimate connexion with the phenomenon just mentioned of phosphorescence of short duration.

In reflecting on the possible explanation of the epipolic analysis of light discovered by Sir John Herschel, I was led to believe that the rays which produced the blue light dispersed in a solution of sulphate of quinine were not the blue rays at all, but the rays of high refrangibility which are mostly invisible. Once this idea is suggested it is easily put to the test of experiment, and the result completely verified the anticipation.

Perhaps the most striking feature in this phenomenon is the change of refrangibility of light which takes place in it, as a result of which visible light can be got out of invisible light, if such an expression may be allowed: that is, out of radiations which are of the same physical nature as light, but are of higher refrangibility than those that affect the eye; and in the same way light of one kind can be got out of light of another, as in the case for instance of an alcoholic solution of the green colouring matter of leaves, which emits a blood red light under the influence of the indigo and other rays. Observation shows that this change is always in the direction of a lowering.

But in speaking of a change of refrangibility I would guard against being misunderstood. All that is intended is that light of one refrangibility being incident on the substance, light of a different refrangibility is emitted so long as the first remains in action. It is not to be supposed, according to a view which has erroneously been attributed to me by more than one writer, but which I never for a moment entertained, much less published, that the refrangibility is changed in the act of reflection from the molecules. The view which I have all along maintained is that the incident vibrations caused an agitation among the ultimate molecules of the body, and that these acted as centres of disturbance to the surrounding ether, the disturbance lasting for a time which, whether it was long enough to be rendered sensible in observation or not, was at any rate very long compared with the time of a single luminous vibration. And now that M. E. Becquerel has shown experimentally by his beautiful phosphoroscope the finiteness of duration of the emission of light in the case of solids in which it was so brief that its emission was described as "fluorescence," as in a solution of sulphate of quinine, there can no longer be any doubt as to the identity of nature of phosphorescence and fluorescence, even though the finite duration of the emission of light after the

incident rays have been cut off has not at present been experimentally demonstrated in the case of any liquid.

I will not dwell further on the nature of this phenomenon, because the subject of the present course mainly confines me to the consideration of light as a means of research. In this point of view it is obvious that as the presence or absence of invisible rays of high refrangibility in a pure spectrum is so easily shown by the exhibition or non-exhibition of fluorescence in a suitably chosen fluorescent body, the phenomenon renders these rays virtually visible, and enables us thereby at once to study the action of bodies upon them, such as the absorbing power of bodies for them. This however constitutes rather an extension of another optical means of discrimination than an independent method. But even as an independent means of discrimination the phenomenon is by no means devoid of application. The property is sufficiently common to be pretty frequently available, and yet sufficiently uncommon to be, at least when taken in all its features, highly distinctive. Experience shows that at least in the case of a single fluorescent substance, as distinguished from a mixture of two or more such substances, the tint of the light emitted, with a given solvent if the active substance be in solution, is usually sensibly

constant whatever may be the refrangibility of the active rays. The tint admits of being observed by suitable methods notwithstanding the admixture of various other bodies with the one in question, even though they should be coloured, provided they are not themselves fluorescent. In this respect it is somewhat superior to the colour due to absorption, the observation of which requires that the coloured body should be at least approximately isolated, so far at least as coloured substances are concerned.

As an example of the use that may be made of the observation of the tint of the fluorescent light, I may refer to a powerfully fluorescent and easily obtained solution which may be made from the bark of the horse chestnut. A decoction of the bark is powerfully fluorescent, as may best be seen by pouring a little into water, but it is liable soon to become brown from tannin &c. contained in it. This may be removed by adding to the freshly made solution a suitable metallic salt, such as a salt of alumina or sesqui-oxide of iron, precipitating by ammonia, and filtering, when the filtrate shows the phenomenon to perfection. Long before the true nature of the phenomenon was known, a glucoside named aesculin had been obtained from the bark, to which the play of colour was attributed. But similar solutions of aesculin show a fluorescence of a

decidedly deeper blue than that of the solution obtained directly from the bark. This shows either that the aesculin is a product of decomposition—a hypothesis which is negatived by the fact that the reagents employed do not affect the tint of the fluorescent light of the solution obtained from the bark—or that in the latter the aesculin must be mixed with some other fluorescent body. In fact the bark of the horse chestnut contains a second glucoside analogous to aesculin, and yielding like it highly fluorescent solutions, which has been named fraxin from its having been first discovered in the bark of the ash. The slightly alkaline solutions of fraxin show a fluorescence which is intermediate between blue and green, and the presence of both bodies in the solution obtained from the bark explains the greater paleness of the blue of the light emitted from it by fluorescence than of that coming from a solution of pure aesculin.

[The fluorescence of solutions of pure aesculin and fraxin obtained from the bark of the horse chestnut, and of a purified solution of the bark itself without any separation of the two bodies, were exhibited by holding burning magnesium wire over glasses containing the three solutions.]

I have already mentioned the searching character of prismatic analysis when applied to the examination of absorption-colours, as compared with a mere examination of the colours by the naked eye. Just

in the same way the observation of fluorescent substances in a pure spectrum exhibits features by which they may be followed and detected in spite of the presence of other substances even in large quantity.

In proceeding in the order of increasing refrangibility, that is from the red to the violet and beyond, the fluorescence is found to commence at a part of the spectrum differing with the particular substance observed, and once commenced to continue from thence onwards. It is frequently however, indeed I might almost say generally, subject to fluctuations of intensity; in one place the fluorescence being copious, and the rays which excited it being soon spent, while in another place it is comparatively feeble, and the rays which excited it, unless they should happen to be absorbed by some other substance present in an impure mixture, are able to penetrate to comparatively great distances in the solution before they are spent. Both these, the place of commencement and the copiousness of emission, form discriminating characters which may be usefully employed. The first is however very much bound up with the colour of the emitted light, and therefore hardly forms an *independent* feature, except in the case of a mixture of two or more fluorescent substances, but in this case is often of great utility.

The second again is bound up with the character of the absorption due to the substance, so that it hardly forms an independent feature when the latter can be observed; but it has this advantage over absorption as a discriminating character, that whereas the latter requires the substance to be approximately isolated from other coloured substances in order that it may be observed, the former can be observed independently in great measure of the presence of such impurities, and enables us in fact to predict the character which the absorption-spectrum of the substance will exhibit when isolated.

I will not dwell further on these phenomena of fluorescence, which are too much of a speciality to be of very general interest. But before I leave the subject of phosphorescence there is one research which I must mention, to which allusion has already been made.

In the instances of phosphorescence, including that phosphorescence of brief duration denominated fluorescence, which have hitherto been mentioned, the phosphorescence was excited by light, or by radiations of the same physical nature as light, though they might not be capable of affecting the eye. But there is another mode of exciting phosphorescence which has been much studied by Mr Crookes and some others. Electric light in all its

forms, abounding as it does in rays of high refrangibility, is specially well suited to excite phosphorescence in most phosphorescent substances. In particular, the beautiful discharge in pretty highly exhausted tubes is well adapted to excite it on bodies placed outside, or still better when that is practicable on bodies within the tube, or on the walls of the tube itself. So far, we have merely a particular case instance of the ordinary mode of exciting phosphorescence. But when the exhaustion is very high a new mode of exciting it comes into action.

We are familiar with the glow surrounding the negative electrode in tubes which are considerably exhausted. When the exhaustion is moderate the glow appears to invest closely the negative electrode. But as the exhaustion progresses it is seen to be separated from the electrode by a dark space, which becomes wider and wider as the residual gas becomes rarer and rarer, until at last it reaches the walls of the tube, and may even at extreme exhaustions fill the whole tube. Now the portion of the walls which lies within the dark space is seen to glow with a phosphorescent light resembling in colour the phosphorescence produced by radiation from the gas glowing under the influence of the discharge with more moderate exhaustions, but usually a good deal brighter; and the same is the case with substances

placed within the tube when the dark space extends over them. Experiments instituted with the direct object of determining whether the phosphorescence in this case is due to radiation or to an actual bombardment of the walls or enclosed substances by molecules projected with great velocity from the negative electrode indicate that the phosphorescence is to be referred to bombardment, provided at least that we do not by the use of that term imply that the action is merely mechanical, but only assert that it is due to the actual transfer, or something accompanying the actual transfer, of the molecules; for it seems probable that the electric discharge, whatever the appropriate idea of that may be, has much to do with it in a direct way. If in this wider sense of the term the phosphorescence is really due to bombardment, that justifies us in speaking of it as a distinct mode of production from the ordinary one in which it is due to radiation.

From the processes to which the tube has to be subjected in order to obtain exhaustions so nearly perfect as is requisite for this observation, the method is pretty well confined to the examination of inorganic substances. In studying the spectra of the transmitted light, Mr Crookes frequently came across a characteristic citron band, evidently indicative of some particular substance. Was it a new element, or

rather compound of a new element, or was it a compound of one of the known elements, including therein a number of new and rare earths recently discovered and as yet imperfectly known? The known elements were tried, and for a long time apparently with a negative result. Frequently the substance Mr Crookes was in search of seemed to be driven into a corner, and yet it managed to slip through the fingers. At last the loophole was discovered, and the substance proved to be the long known but rare earth yttria, in combination with sulphuric acid. Once the origin of the citron band was known, it furnished a very delicate test of the presence of yttria, and the application of this test showed that this rare earth is in reality very widely distributed, though in general in minute quantity. This illustrates what is I believe generally admitted by chemists, that chemical purity represents an unattainable ideal, to which we can only make a more or less near approach in actual experiment.

LECTURE II.

Rotation of the plane of polarization of polarized light produced by various liquids—Its application to quantitative determinations, and to the study of molecular grouping—Magnetic rotation of the plane of polarization—Application to the discrimination between isomeric compounds—Bright lines in the spectra of flames—Application as chemical tests—Discovery thereby of new elements—Connexion between the powers of emission and absorption of the same substance for the same kind of Light—Conditions as to temperature which determine whether a spectral line shall appear as bright or dark.

I HAVE mentioned the rotation of the plane of polarization of polarized light as another property of light which has been turned to account in investigation. It was in the year 1815 that Biot, in pursuing his investigations of the colours developed in polarized light which is subsequently analysed, by the introduction of a crystalline plate which is variously inclined, being desirous of working with inclinations within the crystal greater than could be

obtained when the plate is inclined in air, was led to try the effect of inclining the plate when immersed in oil of turpentine contained in a cell with parallel sides. The oil being of considerable refractive index, approaching that of plate glass, and accordingly that of ordinary minerals, would allow the passage of light within the crystal at an inclination higher than that attained at even a grazing incidence when the plate is in air. But in order to establish the legitimacy of the process it was necessary to show that the oil did not itself act on polarized light. Contrary to what was to have been anticipated, inasmuch as the oil is alike in all directions, differing notably in that respect from a crystal, the oil *did* act on polarized light, and Biot was thus led to the discovery of the action of certain liquids on polarized light. This affords a striking example, of which there are so many, of the way in which the honest pursuit of scientific investigations will sometimes lead to a discovery unthought of by the person who made the experiment with a totally different train of ideas in his mind.

The amount of rotation is found to be different for the different colours, being greater for the more refrangible ones. Accordingly the transmitted light is not wholly extinguished in any position of the analyser, but the different colours are extinguished in

succession, so that on rotating the analyser there is a constant change of colour.

For polarized light of a given refrangibility passing through an active liquid of given kind, the rotation is found to be proportional to the length of path of the light within the liquid, as might have been anticipated. When the liquid is not homogeneous, but consists of a solution of an active substance in an inactive solvent, the rotation is found to be proportional to the strength of the solution. Hence the observed rotation divided by the length of path and by the strength of the solution, is a constant depending on the nature of the active substance, and may be called the specific rotation. The specific rotation, like any other physical constant belonging to the substance, may be used as one of the characters by which it may be known, but its chief value arises in part from the comparative rarity of the rotatory property, so that in many cases there is but one such body liable to be present in the solution, which in that case may be determined quantitatively notwithstanding the presence of other substances; in part from the delicacy of the molecular combinations of which it is able to take cognizance, and of the existence of which it sometimes constitutes the sole evidence.

Let me mention an example of each kind of

application. One of the commonest substances which in inactive solvents yield active solutions, and one which at the same time possesses a high specific rotation, is sugar in its different forms. When sugar is the only active substance liable to be present, and the form in which it occurs is known, we can by simply observing the rotation of the plane of polarization produced by passage of the light through a known length of the solution determine the quantity of sugar present. Accordingly an instrument designed for effecting this measurement with accuracy and facility has been named a saccharimeter.

A rather amusing application of the saccharimeter which has been proposed and I believe actually employed in some foreign country is for excise purposes, in fixing the duty on beer, which according to a law there in force depends on its alcoholic strength. In distilled spirit we have practically only two substances liable to be present, namely, water and alcohol, which are of very different specific gravities, so that the specific gravity of the mixture, which can be easily and rapidly taken by the hydrometer, determines the strength. But in beer the specific gravity depends not only on the alcohol, which lowers it, but on the sugar, or rather dextrine, which raises it. (The dissolved substances other than sugar may be neglected, as being present in no great quantity.) Hence the

specific gravity gives only one relation between two unknown quantities. But the amount of sugar can be determined by measuring the rotation of the plane of polarization, and knowing this we can calculate what the specific gravity would become if the sugar were removed, and from thence deduce the alcoholic strength.

As an instance of feeling, so to speak, a delicate molecular combination by the observation of the azimuth of the plane of polarization of polarized light, I will refer to the behaviour of a freshly made solution of grape sugar or glucose. This substance crystallizes by itself, i.e. with merely water of crystallization, and curiously enough forms also a definite crystalline compound with common salt. The crystals of either kind dissolve readily in water, and the solutions when once made remain apparently unchanged. Nevertheless when the rotation of the plane of polarization produced by the solution of either kind of crystals is immediately measured, it is found to be nearly double that of a solution of glucose of the same strength which has been made some time. The rotatory power of the fresh solution gradually diminishes, and in the course of seven hours or so at ordinary temperatures reaches a permanent value, a change which takes place at once on boiling. Had this change occurred only in the solution of the

crystallized compound of glucose and common salt, we should naturally have inferred that the compound at first dissolved as such, but was of an unstable character, and this may possibly actually be the case; only if it be so some analogous change must take place when we have nothing present but glucose and water, and what the naturo of the change of molecular grouping in that case may be we do not know. It is remarkable that cane sugar, though so closely allied to glucose, shows no such phenomenon.

As another example of the application of this method to the study of the mode of combination of bodies in mixed solutions, I may refer to an elaborate research by Dr Jellett, on the combinations formed when a vegetable alcaloid is present in a solution containing two acids, with either of which the alcaloid could combine.

It was in the year 1845 that Faraday made the remarkable discovery that uncrystalline bodies, or at least those of high refractive power, when under the influence of a powerful magnetic force act on polarised light. The action consists in a rotation of the plane of polarization, agreeing so far with the natural action of liquids like sirup of sugar, from which however it differs in the circumstance that whereas in the latter the action is alike in all

directions, in the former the amount of rotation varies with the direction of the light. It is greatest when the light travels in the direction of the lines of magnetic force, and vanishes in a direction perpendicular to those lines, and the direction of rotation is reversed on passing across the equatorial plane, or plane perpendicular to the lines of magnetic force.

It has been found that for a given substance and kind of light the rotation is proportional to the length of path, to the magnetic force, and to the cosine of the inclination of the path to the lines of force, from whence by measuring the rotation under given circumstances we can determine the specific rotation, or more readily the ratio of the specific rotations of two substances—more readily because in this case we do not require the magnetic field to be sensibly uniform (or else calculable, so as to permit us to perform an integration) and the magnetic force known; it is sufficient that the magnetic field and the force in it be the same in successive experiments.

The specific magnetic rotation, like the specific gravity, the refractive power, &c. is a physical constant giving one of the characters of each particular substance. An elaborate comparison of the specific rotations of several chemical groups has recently been made by Mr Perkin, with the result among

others that the difference between isomeric compounds is in many cases clearly revealed by a difference in the values of *this* constant, whereas most other physical constants are nearly alike for the two.

It appears therefore that the rotation of the plane of polarization produced by the action of magnetism on bodies across which light is proceeding, like the natural rotation belonging to such bodies as sirup of sugar, &c. is capable of laying hold of and revealing delicate differences of molecular grouping. It is less easily observed than the natural rotation, from which also it differs in being of general instead of exceptional application.

In all the phenomena which I have brought before you in my last lecture and in this, and indeed in all that I shall have occasion to mention in this year's course, there is a very intimate relation between molecular grouping and the optical features observed. We touch here on the boundaries of our present physical knowledge. That light consists in the vibrations of a subtile medium or *ether*, that self-luminous bodies, including phosphorescent bodies, which are for the time being self-luminous, are in a state of molecular agitation which they are capable of communicating to the ether, that consequently in the phenomenon of absorption molecular disturbance

is excited in bodies at the expense of etherial vibrations—all this is so well established as to leave no reasonable room for doubt. But what may be the precise nature of the molecular vibrations, what may be the mode of connexion by which the vibrations of the ether agitate the molecules, or the molecules in their turn are able to agitate the ether, what may be the cause of the diminished velocity of propagation in refracting media, what may be the mechanical cause of the difference of the velocity of propagation of right and left-handed circularly polarized light in media like sirup of sugar, which is manifested by a rotation of the plane of polarization of plane-polarized light, still more what may be the nature of the action of magnetism in respect of the propagation of light through bodies—all these are questions concerning the true answers to which we can affirm nothing, though plausible conjectures may in many cases be framed.

We have seen how searching are the phenomena of light with respect to the molecular constitution of bodies, although I have said nothing relating to the information they afford concerning the molecular structure of crystals. The molecular groupings on which I have chiefly dwelt as illustrated by the phenomena of light are mostly of a complex character, chiefly belonging to organic substances, and of a

kind which could not exist at a high temperature. The subject which I propose next to bring before you relates almost exclusively to bodies in a state of incandescence, so that organic combinations are excluded. If on the one hand the field of research is thereby limited, on the other the nature of the phenomena connects them with other branches of science, rendering the subject of more general interest.

It has long been known that salts of particular metals, such as those of the alkalies and alkaline earths, when introduced into a feebly luminous flame, such as that of a spirit lamp, cause a coloration depending on the base of the salt. Thus salts of potash give a violet, of soda a yellow, of lithia a red, of baryta a green, of strontia a red, of lime a brick red. The colours thus produced have to a certain extent been used by mineralogists in discriminating between different minerals by the aid of the blowpipe.

But just as the prismatic analysis of the colours due to absorption reveals characters of the absorbing body which are often highly distinctive, but which would escape detection so long as we merely observed the absorption colour with the naked eye, so here a prismatic analysis of the coloured flames reveals immensely more than can be perceived merely by

looking at the colour. As long ago as 1834, the late Mr Fox Talbot showed that the red due to a salt of strontia and the red due to a salt of lithia can be at once distinguished by the prism, which in the case of lithia shows a narrow well-marked line in the red, not far from the line C, while the spectrum of a strontium flame wants this line, and is of a more complicated character. Several years before, both Sir John Herschel and Mr Talbot had drawn attention to the characteristic lines produced on introducing the salts of certain bases into flames, and had pointed out how small a quantity of a substance suffices to produce the effect, though it is true that Talbot entered into some erroneous conjectures as to the origin of the bright line D.

It is remarkable for how long chemists neglected the precious means of discrimination lying at their very hands in the use of the prism—a striking example of how much may be lost by a too exclusive devotion to one branch of science to the neglect of others. Notwithstanding that W. A. Miller had published maps of the spectra of flames coloured by the alkalies and alkaline earths, it was not till Bunsen and Kirchhoff published their celebrated researches that spectral analysis came into regular use with chemists.

Bunsen and Kirchhoff engaged in a methodical

chemico-optical examination of the spectra of salts introduced into an almost non-luminous flame, for which they used the flame of a mixture of air and coal gas, burnt in a Bunsen's burner, taking the greatest care to purify the substances used, and examining separately the spectra given by the same base combined with a variety of acids. They found that the spectra depended on the metal of the salt, and not on the acid radical; the acids which were sparingly volatile merely showing the spectra more feebly. The spectra showed bright lines or narrow bands characteristic of the metals; the heavy metals as a rule showed no spectra when examined in this way. The order of importance of the various bands in the spectrum of the same metallic salt as tests of the presence of the metal was determined, and thus it became easy to detect even small quantities of these metals present in a mixture.

These investigations were almost immediately rewarded by the discovery of two new elements, Rubidium and Caesium, which were traced by the appearance of certain bright lines in the spectrum of a Bunsen flame when a variety of specimens of substances from different localities, including waters from mineral springs, were introduced into it. They proved to be the metals of two oxides of the group of alkalies, and were named after the colour of their

most distinctive bands. Nor was this all. The facility of the test, which though indicated long before had not been put in practice, enabled Bunsen and Kirchhoff to show that lithia, which previously had been regarded as a rare substance, was on the contrary very widely distributed, though usually present in small proportion; and led to the discovery of sources and means of separation of the alkali by which it can be obtained at a comparatively cheap rate.

The method of spectrum analysis, carried out as above indicated, or modified by the employment of a succession of electric discharges instead of a Bunsen flame, has led in the hands of others to the discovery of three more new metals, namely Thallium independently by Crookes and Lamy, Indium by Reich and Richter, and Gallium by Le Coq de Boisbaudran.

In their original paper, Bunsen and Kirchhoff contented themselves with establishing the fact that different salts of the same metal when introduced into a Bunsen flame gave the same spectrum, which could therefore be used as a test of the presence of the metal; they did not commit themselves to any theory as to what the particular vapour present in the flame might be which produced for each metal its characteristic spectrum. That it was a vapour of some kind, follows both from the cir-

cumstances of the experiment, and from the consideration that it is only in the state of vapour that substances exhibit such narrow absorption bands as are actually produced by the flames, or as would correspond to the bright lines in the spectrum of the light they emit. It might be supposed that as different salts of the same metallic oxide yield solutions which as a rule exhibit similar modes of absorption, so different salts when volatilized in a flame might yield vapours consisting of the salts themselves, and yet having a spectrum in common. Or it might be supposed that the identity of the spectra was evidence that the salts were decomposed in the flame, and that the glowing vapour which yielded the spectrum common to them all was that of the metal itself.

But a different conclusion resulted from the observations of Alexander von Mitcherlich on the spectra of the chlorides, bromides, iodides and fluorides of the alkaline earths. When a bead of a chloride for instance is introduced into a Bunsen flame, in the manner followed by Bunsen and Kirchhoff, a spectrum with the bands of the salts in general of the same base is obtained. But as in this mode of observation a minute quantity of the volatilized chloride is present in an atmosphere in which there is plenty of oxygen and vapour of water, it may very well be that the chloride is decomposed with the formation of an

oxide and hydrochloric acid. To prevent this von Mitcherlich used a solution of the chloride to which a comparatively large quantity of sal ammoniac, which itself gives no spectrum, was added. In this way the volatilized chloride was present in an atmosphere abounding in hydrochloric acid (from the temporary dissociation of the sal ammoniac) and was accordingly maintained as such, and now the spectrum showed bands indeed, agreeing so far with the spectrum obtained in the former manner, but the actual bands were quite different. Accordingly we must infer that in this case the glowing vapour was the chloride, but in the former method the oxide.

The other haloid compounds behaved in a similar manner, showing spectra differing from each other, and from that of the oxide. Moreover there was a remarkable similarity of character between the spectra of the chloride, bromide and iodide of the same metal; a group for instance of bright lines in the chloride having corresponding to it in general arrangement, but differing a little in position, a group in the bromide, and the latter again having corresponding to it a group in the iodide. Moreover the order of the change corresponded to the chemical order, the bands of the bromide being intermediate between those of the chloride and those of the iodide, just as in its chemical relations bromine is inter-

mediate between chlorine and iodine. We may infer that the vibrating molecular systems which disturbed the ether, and were thus the source of the light, were the compounds, the chloride, bromide and iodide, of the metal; and we have here another example showing how closely the optical phenomena presented by bodies touch their molecular structure.

The same conclusion results from a comparison of the spectra of ordinary flames into which certain salts are introduced with that obtained from the more powerful incandescence produced by electricity. When an electric discharge is passed between electrodes formed of the metals themselves, or electrodes wet with solutions of their salts, spectra are obtained which in some cases are quite different from what are shown by flames. In the case of the alkalies, the same lines are seen as in flames, with the addition of others of similar character. But in the case of the alkaline earths, in lieu of the bands seen with flames, narrow bright lines are seen, at least with the jar discharge, occupying different positions from the bands. The augmentation in the number of lines brought out in the first case, that of the alkalies, is explicable by the greater intensity of the incandescence, and we infer accordingly that the lines seen in the flame spectra are those due to the metals themselves, of which the oxides are the alkalies,

while the difference in the spectra in the second case indicates that the bands seen in the flame spectra of the alkaline earths are referable to some compound of the metals, and accordingly, as the burning takes place in air, to the oxides, that is, to the alkaline earths themselves.

The methods by which the chemist judges of the nature of the bodies with which he has to deal are restricted to such substances as he can get access to, so as to subject them to his manipulation, but light may be examined at any distance from its source which is not so great as to render it too feeble for observation; and when the source is one of great intensity and magnitude, that distance may be enormously great. Hence such information as may be obtained from the character of the light respecting the chemical nature of the source from which it comes is available for the examination of bodies at a distance. This leads me into a field of great interest, for a reason which I will not anticipate by mentioning at present.

The celebrated Fraunhofer, after whom the fixed lines of the solar spectrum have been named, applied the admirable prisms and other optical apparatus of his own manufacture to the examination of other sources of light. He found that while the same system of dark lines is seen in the light of the sun,

whether coming directly, or reflected from the clouds or from the moon or planets, so far as the feebler light in the latter case admitted of a comparison, the fixed stars showed systems of dark lines differing from the solar system and from one another, though having some of the lines common to two or more of the systems. This afforded confirmation from an unexpected quarter of what was already well established on astronomical grounds, that the moon and planets shine by light derived from the sun and reflected by those bodies, whereas the stars like our sun are independent and self-luminous.

Fraunhofer found further that artificial sources of light in some cases show bright lines in their spectrum. The discharge of an electric machine in action showed a whole system of bright lines. He further observed that the light of a candle shows a bright line in the yellow *exactly coinciding* in position with the dark line D of the solar spectrum, and like it double. Fraunhofer did not advance any hypothesis to account for this coincidence, which is too remarkable to be attributable to a casual similarity of position so close as to appear to be an identity.

This conclusion long remained an isolated fact without explanation. But in the year 1849 Foucault made a remarkable step in advance. He was engaged in comparing the brightness of the electric

light with that of the sun; and in the course of his researches he had occasion to send a reflected beam of solar light across the arc which connected the carbon poles, using a lens to form an image of the sun at the place of the arc. He noticed that in the solar beam which had traversed the arc the double dark line D appeared darker than usual, while when the sun's light was intercepted, and the only light was that of the arc, a bright line was seen in the same place. When the sun's image was arranged to cover a part only of the arc, so that the light passing through one part of the slit was derived from the arc alone, and that passing through the rest consisted of the light from the arc together with the solar light which had passed through it, the strip of the spectrum corresponding to the first part of the slit showed the bright D, while the remainder of the spectrum, corresponding to the compound light, showed a dark D which was *an exact prolongation* of the former, demonstrating the complete coincidence of position of the dark and bright D. Moreover the dark D could be obtained independently altogether of solar light by sending through the arc by reflection the light from one of the incandescent poles. The glowing poles alone give a continuous spectrum, and the spectrum of the compound light obtained as above described showed a dark line D just like the

solar spectrum. This shows that the electric arc, which emits light of the definite refrangibilities of the two components of the Fraunhofer line D, acts also as an absorbing medium which absorbs light of the same definite degrees of refrangibility.

I was very much struck with this observation when Foucault mentioned it to me a few years later in conversation. It seemed to me that a dynamical illustration of how a medium could act both by emission and absorption for light of a definite refrangibility was not far to seek. Imagine a series of stretched wires like pianoforte wires all tuned to the same note. The series if agitated, suppose by being struck, would give out that note, which on the other hand it would be capable of taking up if sounded in air. To carry out the analogy, we have only to suppose a portion of the molecules constituting the vapour of the arc to be endowed with a capacity of vibrating in a definite manner, that is, according to a definite time of vibration.

But what were these molecules? It is well known that the bright D in flames is specially characteristic of compounds of sodium, though from its very general occurrence some had doubted whether it were not really due to something else. But in what condition must we suppose the sodium in the arc to be? The compounds of sodium, such as common salt,

carbonate of soda, &c., are colourless; and it would be contrary to the analogy of what we know as to the relation of gases and vapours to their liquid or solutions to suppose that a gas which does exercise absorption should be merely the vapour of a heated solid which does not. On this ground it seemed to me that the substance which exercised the selective absorption in Foucault's experiment must be free sodium. This might conceivably be set free from its compounds in the intense actions which go on in the sun or in the electric arc; but I had not thought that a body of such powerful affinities would be set free in the gentle flame of a spirit lamp, nor perceived that the fact of that flame's emitting light of the definite refrangibility of D entails *of necessity* that it should absorb light of that same refrangibility.

To enable you the better to follow what I have now to bring before you, it will be well to enter on a brief digression.

Conceive a closed opaque envelope of any kind to be uniformly and permanently heated. If a thermometer be introduced into the envelope, the mercury will begin to rise, but soon the rising will become slow, and presently the mercury will have risen to a height which no longer alters, and which depends on the temperature of the envelope.

Now how is it that the thermometer receives the

heat which causes the mercury to rise? Partly no doubt it is through contact with the heated air inside the envelope. The air in contact with the bulb gets cooled and descends, giving place to warmer air which in its turn warms the thermometer; while the air which was cooled by contact with the thermometer gets warmed by contact with the envelope, and so becomes ready to warm the thermometer when it gets to it. But this is far from accounting for the whole effect, for the rise of temperature of the thermometer takes place equally when the envelope is exhausted as completely as possible of air, though not, it is true, so rapidly. Consequently a large part of the rise of temperature must be attributed to the radiation of heat from the walls of the envelope, across the intervening space, to the bulb of the thermometer.

But what takes place when the thermometer has come to its permanent condition? Are we to suppose that the radiation then ceases? We have the strongest reason for believing that the radiation of heat is perfectly analogous to the radiation of light, and consists in the communication of a disturbance from the ponderable molecules to the ether. But the envelope is now just in the same state as before, that is, as it was when the mercury of the thermometer was still rising, and the ether is there as before

to receive the motion communicated to it. The molecules of the envelope cannot prophesy what is ultimately to become of the motion they may communicate to the ether, and regulate the communication accordingly. We must suppose the radiated disturbance to proceed from the walls to the bulb just in the same manner whether the bulb be hot or cold. Falling on the bulb, it will be in part absorbed by it in the same manner in the two cases. How is it then that the temperature of the bulb does not go on rising? Why, just as the envelope radiates towards the bulb, the bulb radiates towards the envelope. The amount radiated will depend on the temperature, the nature of the bulb being supposed given; the higher the temperature the more copious will be the radiation. If the bulb gains in temperature by the heat that it receives by radiation from the walls, it loses in temperature by the heat which it radiates towards the walls. The actual change is merely the balance between the two; and thus we are led to infer that even when everything has come to its permanent state, and there is apparently perfect rest within the envelope, there is still a radiation and absorption of heat going on; the bulb radiates heat towards the envelope, and receives heat from it by its radiation; and as the thermometer remains steady, we infer that there is an exact balance between the loss of heat on

part of the bulb by radiation and its gain of heat by absorption. The steady height at which the thermometer stands is found to be the same wherever the thermometer may be placed within the enclosure, and whatever may be the nature of the covering, if any, of the bulb, and to be that marking the temperature of the envelope as determined by other means.

But while the nature of the covering of the bulb does not affect the ultimate height at which the thermometer stands, it affects most materially the rate at which that ultimate height is approached. If the bulb be coated with lamp black, the thermometer rises more promptly, if with silver more sluggishly. We infer from this, what we know by independent means, that silver absorbs radiant heat less readily than glass, and glass again than lamp black. And as in each case there is a perfect balance between the heat absorbed and the heat radiated, we infer that the absorbing power of a substance at any temperature corresponds to its emitting power at the same temperature.

The theory above mentioned of the mode by which the equalization of temperature is effected, and of the action that still goes on when the temperature has become uniform, is due to Prevost, and is called Prevost's theory of exchanges.

Hitherto we have spoken only of the total quan-

tity of heat radiated or absorbed, without regard to quality. But we know that radiant heat like light consists of kinds differing from one another in refrangibility; and just as a body may be transparent for light of one kind and opaque for light of another, comparatively speaking at least, so a body may be comparatively transparent for heat of one kind and opaque for heat of another. Now a very important extension of Prevost's law of exchanges was made by Professor Balfour Stewart, who instituted a series of experiments on the quality of the heat radiated by rock salt and other bodies, as tested by the capacity of the heat to pass or not to pass through plates of the same substance as that which emitted the heat. He showed for instance that the heat radiated by rock salt is in good measure absorbed by a plate of rock salt, a body which so freely transmits most kinds of heat that it is sometimes regarded as perfectly diathermanous. All his results he showed to be in accordance with an extension of Prevost's theory of exchanges, according to which the balance between radiation and absorption holds good, not merely for heat taken as a whole, that is, for all kinds taken together, but for each particular kind of heat in particular.

The transition is easy from heat to light; in fact we have the strongest reasons for believing that

physically considered there is no greater difference between radiant heat and light than there is between light of one colour and light of another. Conclusions of a similar nature concerning light accordingly naturally presented themselves to the mind of Professor Stewart; and he was on the point of publishing some experiments in this direction when he was anticipated in the extension of Prevost's theory to light of each kind in particular, and in the more complete experimental establishment of the legitimacy of that extension which the facility of observing light renders possible, by Professor Kirchhoff, who had been led to it quite independently of what Professor Stewart had done in heat, and who deduced from it most important consequences.

In the year 1860 Kirchhoff, who was engaged along with Bunsen in a very important series of researches to which I have already had occasion to allude, discovered that when light from a source at a sufficiently high temperature was passed through a flame which showed the bright line D, in consequence, as he and Bunsen had now conclusively proved, of the presence of sodium, in some state, a dark line D was artificially produced by absorption of the light which was passed through the flame. He was not aware at the time that the same thing had been discovered many years before by Foucault in the

particular case of the electric arc. He however was led to generalize the phenomenon, and to affirm that flames which show bright lines or bands in their spectrum must for that very reason act as absorbing media, absorbing light of the same refrangibilities sent through them. Whether in the spectrum of the compound light, consisting partly of the light emitted by the flame, partly of the light sent through it, the line shall appear as bright on a dark ground or as dark on a bright ground, depends on the relative temperatures of the flame and of the source from whence the light sent through the flame proceeds. If the flame be the hotter of the two, and we contrast the region of the line with the parts of the spectrum immediately on both sides of it, there is more gain of light by emission from the flame than loss of light by absorption of light sent through it, and the line therefore appears as bright on a less bright ground. If on the other hand the source is the hotter, and is opaque, at least in the region of the line and its neighbourhood, so as to give the full radiation due to its temperature, and the light comes from it direct, not weakened by reflection or otherwise, then the gain of light by emission from the flame is more than compensated by the loss by absorption of light coming from the source, and the line is seen as dark on a brighter ground.

We have hitherto considered two independent sources of light, one the flame, the other the source of light which sends its light through the flame. But the different parts of a flame are not all at the same temperature; the outer mantle is cooler than an envelope inside it which is the chief seat of the combustion. Accordingly in certain cases the outer envelope takes the place of the flame in the above explanation, the inner shell that of the body of higher temperature which sends its light through it. Thus when a Bunsen flame is richly fed with chloride of sodium, and the spectroscope is one of high dispersion, each component of the D line is seen considerably widened, and in the middle of each is seen a hair-like dark line, giving a pair of dark lines just as in the solar spectrum.

LECTURE III.

Inferences deduced from a study of the dark lines in the solar spectrum as to the presence of certain chemical elements in the sun, and as to the condition of that body—Spectra of the stars, including the examination of the ultra-violet region—Resulting classification of the stars—Nebulae—Character of their spectra, and inferences thence derived as to their constitution—Examination of the star which burst out in the Northern Crown in 1866*—Comets, and character of their Light—Theory, in some respects new, of these bodies.*

Of the dark lines of the solar spectrum, some are attributed to absorption of light in the atmosphere of the earth, and accordingly become much more conspicuous when the sun is very low. These however appear to form a small minority, and to lie mostly in the less refrangible portion of the spectrum. The others must be attributed to the quality of the light as it comes to us from the sun. It is only through the absorption of light by vapours that we are able to imitate the solar spectrum in this respect,

that we get a spectrum interrupted by numerous dark lines or narrow bands. This consideration points to absorption in the sun's atmosphere as the probable source of the lines; and the relation between absorption and emission brought out so clearly by Kirchhoff leads us to seek among vapours which show bright lines by emission for coincidences of position between such bright lines and dark lines of the solar spectrum.

In the discharge of electricity, whether between the poles of a powerful battery, or more conveniently between electrodes connected with the secondary terminals of an induction coil, we have a source of temperature far surpassing what can be obtained merely from flames, and which is competent to volatilize even such refractory substances as iron and platinum. It might be that in this way we should obtain such evidence of coincidence of position between bright lines artificially produced and the natural dark lines of the solar spectrum as to lead us to the conclusion that the substance volatilized in the discharge was also present in the atmosphere of the sun as an absorbing vapour.

But the number of dark lines in the solar spectrum is extremely great, and the same is true of those obtainable from the electric discharge passed between different substances. A coincidence or apparent co-

incidence of position between a bright and a dark line *might* therefore be merely casual, and not indicate any real physical connexion. I say "or apparent coincidence," because our means of judging of the exactness of what appears to be a coincidence are finite. The greater the precision with which we can judge of coincidences of position, the greater is the probability that an apparent coincidence is a real one, and indicative of a physical connexion. And the probability would be enormously increased if the substance under examination should be found to show not merely one but a series of bright lines having, apparently at least, coincident with them a corresponding series of dark lines in the solar spectrum.

The sharpness with which we can compare the coincidence of position of a bright artificial line and a dark solar line, whether seen simultaneously or successively, in the same spectrum, depends on the purity and angular extent of the spectrum. For the sake of subjecting the exactitude of any apparent coincidences to a very searching scrutiny, Kirchhoff was led to construct a most elaborate and detailed map of the solar spectrum, at least of the brighter part of it in the first instance, surpassing anything of the kind that had been done before. The map contains for comparison the places of the bright lines

seen when an electric discharge is passed between electrodes of various metals.

In several cases the coincidences were so striking that there could be no doubt of the reality of the connexion, and consequently of the presence of the substance in question in the atmosphere of the sun.

Let us take for example the particular case of iron. The bright lines in the spark spectrum of iron are very numerous, and yet these lines are sure to have corresponding to them in position dark lines in the spectrum of the sun; and not only so but in general the lines which are strong in iron have strong dark lines agreeing with them in position in the spectrum of the sun.

Let us pause for a moment to reflect on the conceptions which these results open out to us of the state of incandescence of the sun. That it is intensely hot, must have been inferred from the earliest times from the brilliancy of its light and the warmth which we experience when it shines on us. But this does not furnish us with any estimation of *how* hot it is, or even enable us to say what effects at least its temperature must be capable of producing. The observations I have last mentioned show that even its outer and somewhat cooler portions must still be above the *boiling-point* of iron, hot enough, that is, to maintain iron in the condition of a per-

manent gas. And if even the outer portions are at such an enormous temperature, what must be the condition of the interior?

It is true that some physicists, some even of great eminence, have speculated on the body of the sun being of a comparatively moderate temperature, and have supposed that the intense luminosity was confined to an envelope, the so-called photosphere, forming the lower portion of the sun's atmosphere; though it must be noted that the term "photosphere" is employed also by those who do not share the theory of a comparatively cool nucleus. This strange theory, so contrary to all that we know as to the behaviour of a body within a heated envelope, was probably devised to account for the phenomena then known as to the behaviour of the dark spots on the sun's surface. But besides the inherent improbability of any such hypothesis, later research has revealed phenomena of the spots which ill accord with the idea of a comparatively dark solid nucleus. And we shall see presently that we have evidence of tremendous actions going on in the sun, of a piece with the enormous temperature which even from the observations already mentioned we must attribute to it.

Besides sodium and iron, the first researches of Kirchhoff indicated magnesium, calcium, chromium,

nickel, perhaps cobalt, and probably barium, copper and zinc, as present in the atmosphere of the sun. Later researches conducted, partly by further carrying out the same method, partly by following the guidance of new theoretical views, have considerably added to this list.

But there is one element, chemically indeed analogous to a metal, but very different from the metals as regards the physical condition in which these substances commonly present themselves, the indication of the presence of which in the sun must particularly be mentioned: I allude to hydrogen. When an induction discharge is passed through a Geissler's spectral tube containing rarefied hydrogen, and the spectrum of the bright discharge in the capillary part is examined, it is seen to consist mainly of three bright lines or narrow bands, for they are not mere lines, with a fourth further on in the violet. Now all four coincide exactly in position with four conspicuous dark lines in the solar spectrum, of which the first two were among the standard lines selected by Fraunhofer, namely, C in the red and F in the blue. We infer therefore that hydrogen is present as an absorbing gas in the sun's atmosphere; and what confirms the correctness of the conclusion is that in the spectra of others of the heavenly bodies the lines C and F are usually seen

to go together, that is, to be present or absent together.

It may be said, How can we suppose that hydrogen thus exercises an absorbing influence, seeing that when examined on earth, even in considerable lengths, it appears perfectly transparent?

Of course the distance that the sun's rays must travel through hydrogen in the sun's atmosphere if hydrogen be there must be enormous compared with anything that we can imitate by experiments on earth; and it may be that very great lengths of transit are necessary to bring out the distinctive absorption due to hydrogen. Yet considering how small a thickness of the gas suffices to bring out in great intensity the bright lines of the gas, and how small a thickness of vapour of sodium or lithium suffices to show the selective absorption of the vapours of those metals when a salt of one of them is introduced into a Bunsen flame, this explanation seems open to a good deal of doubt; though on the other hand the fact that a selective absorption has recently been shown to take place in oxygen when great thicknesses are looked through, though in moderate thicknesses the gas appears to be perfectly transparent, shows that the explanation is far from impossible. It is to be noticed however that the equality between emission and absorption for each kind of

light in particular can only be affirmed as of necessity when the gas is of the same temperature as the opaque body which emits the light that passes through it. Should the absorbing power of a gas for light of a given kind alter with the temperature of the gas, as may well be the case when we have to deal with such enormous differences of temperature as that obtained by an electrical discharge and ordinary temperatures, the result mentioned might very well follow; the gas might be transparent at the low temperature, and yet might exercise a selective absorption at the higher. Or again it is conceivable that the molecule of hydrogen may be temporarily split up by the discharge of an induction coil, or permanently so by the enormous temperatures which prevail in the sun, and that it is the product of dissociation which exercises the selective absorption. In any of these ways the difficulty that has been mentioned may admit of explanation, though we are not able at present to say what the true explanation is.

I mentioned in my last lecture that Fraunhofer observed that different fixed stars show different systems of dark lines in their spectra. Now that we are able, to a considerable extent at least, to connect these dark lines with the presence of particular elements, we are enabled by spectroscopic

observation to gain some information respecting the chemical constitution of these bodies, situated though they are at distances from us of which we find it difficult to get an adequate conception. No one has distinguished himself by working in this field more than our own countryman Dr Huggins, the firstfruits of whose labours, at that time in conjunction with the late Dr W. A. Miller, are published in the Philosophical Transactions for 1864. Dr Huggins has since continued his labours in this field, and after encountering many difficulties has successfully applied photography to the delineation of the spectra of the brighter stars in the more refrangible part of the visible spectrum, and in the ultra-violet region where eye observation fails altogether. The results are published in the Philosophical Transactions for 1880.

The general character of the results of the whole investigation is very striking. While as was previously known the stars agree with one another, and with our sun, in the character of giving a spectrum interrupted by dark lines, these more extended observations—more extended as embracing a region inaccessible to the eye used directly—point towards a classification of stars on the whole in approximate order of sequence. The white stars, which as he had previously found show very strongly the dark lines C and F,

in the visible spectrum, which are attributed to hydrogen, showed a whole series of dark bands, as many as 12 in α Lyrae, which were arranged in a very regular manner, decreasing in width and distance apart in going in the direction of increasing refrangibility. They were the same in the different stars, except that in some a few of the more refrangible lines could not be seen in the photographs. They gave a decided appearance of law, as if they belonged to one another, and to the visible lines C and F. Moreover the first five of them, of which the first two belong to the still visible but weaker part of the visual spectrum, have been identified in position with the bands of incandescent hydrogen. There appears to be very little doubt that they belong to hydrogen, which in these stars shows itself more strongly than in our own sun. In these stars the other dark lines, mostly referable to metals, are fine and inconspicuous. On the other hand the bright but reddish star Arcturus shows a complicated spectrum more nearly resembling in character that of the sun, but in some respects differing from the spectra of the white stars even more than does that of our sun. And Secchi found that the spectra of the decidedly red stars, which are all small, showed shaded bands analogous in character to some of the spectra artificially produced by the electric discharge, the origin of which, that is, whether they

belong to elements or compound bodies, is still, in some cases at least, a matter of dispute.

It is noteworthy that some of the stars show among their more conspicuous lines series answering to those of elements which are by no means common on the earth, and which appear to be either absent or not prominent in the sun. Thus in the two stars the spectrum of which is figured by Huggins and Miller in the Philosophical Transactions for 1864, namely Aldebaran and α Orionis, there are several coincidences in both cases with each of the elements bismuth and tellurium.

The general result tends to establish a similarity of plan, combined with individual differences, between the different fixed stars, and between them and our own sun.

We can hardly avoid surmising that these distant suns may, like our own sun, be accompanied by planets circulating around them, and that these planets again, or such of them as may be habitable, are like our own earth tenanted by living beings, it may be by rational beings of some kind.

I come now to another class of heavenly bodies which have long excited the interest and curiosity of astronomers, and on the constitution of which spectroscopic research has of recent years cast a new and unexpected light; I allude to the nebulae.

When the heavens are examined with a good telescope, here and there among the stars are seen bodies differing from the stars in the circumstance that their *light is not concentrated into a point*, but is more or less diffused, and subtends, as seen through the telescope, a very appreciable angle. If seen they could not ordinarily be recognised with the naked eye, but the hazy appearance of the great nebula in Orion may at this time of year be noticed any clear evening even with the naked eye. Among them are several which show a round outline, from whence they have received the name of planetary nebulae.

Now what are these nebulae? Are they luminous bodies of a really continuous structure, or are they merely clusters of stars, either so minute, and comparatively speaking near together, or so distant, and apparently near together, that they cannot be distinguished individually? We are familiar with the gleam of light which constitutes the milky way. We cannot with the naked eye resolve it into stars, yet with the telescope it is so resolved; may it not be that even the nebulae are merely clusters of stars which our telescopes are unable to resolve?

Sir William Herschel was of this opinion, and supposed that our sun and the brighter stars which we see were merely the portions comparatively near the centre, while the milky way formed the distant

outlying portions, of a vast flattened cluster of stars, which at an enormously distant point in the universe might as a whole appear as a nebula, like what the planetary nebulae appear to us; and conversely, that the planetary nebulae were groups of a somewhat similar constitution, which cannot be resolved, and gigantic as the linear magnitude of the systems may be subtend as a whole merely a small angle, in consequence of the almost inconceivably great distance at which we see them. Laplace on the other hand supposed that the nebulae were really diffuse luminous matter in process of forming suns by the condensation due to the mutual gravitation of their parts. In many cases the nebulae exhibit stellar points in an approximately central position; too frequently to allow us to suppose that they are merely stars which happen to be in the same direction as the nebulae, but which may really be immensely nearer to us or further from us than the actual nebulae. According to Laplace's views, these stellar points would be stars in process of formation.

As telescopes were improved in power and sharpness of definition, our ability to resolve clusters of close stars was naturally increased. In particular, when the magnificent six-foot reflector constructed by the late Earl of Rosse was applied to the scrutiny of the heavens, what had formerly appeared as patches

of light were in some cases resolved into clusters of stars, while in other cases they still resisted resolution. I recollect long ago at a meeting of the British Association hearing the late Dr Robinson, in the course of some remarks on the results obtained through Lord Rosse's telescope, state that the balance of probability seemed to him now to lie in favour of the supposition that if some nebulae still appeared to be continuous in constitution, though showing the structure, frequently of a spiral or wisp-like character, which that wonderful instrument revealed, it was only because in spite of the great advances which had been made our instruments were still of finite resolving power. In this unsettled state the subject remained for many years, until an unexpected discovery set the question at rest.

Immediately following the paper by Dr Huggins and Professor Miller in the Philosophical Transactions for 1864 to which I have already alluded, is a paper by Dr Huggins on the spectra of the nebulae. In this he relates how on turning the Royal Society's telescope which was entrusted to him to one of the planetary nebulae, he was surprised by finding its spectrum to consist of three isolated bright lines, of which the first coincided in position with a line of nitrogen, the third with the line F of hydrogen, while the intermediate line did not agree with that of

any known element, though it lay near a line of barium. A number of nebulae to which the instrument was directed showed the very same spectrum, except that the more refrangible and fainter of these lines was frequently invisible, while on the other hand in one case a fourth, more refrangible line was faintly seen, which coincided with the line of hydrogen near G. These nebulae frequently contained stellar points, corresponding to which was a narrow continuous spectrum, interrupted probably by dark lines which there was hardly light enough to see.

Now as it is only when matter is in the state of gas or vapour that when rendered glowing it gives out a spectrum with isolated bright lines, we have a right to conclude that these nebulae, making abstraction of the stellar points, consist of glowing gas.

But what is the gas? This is a question not so easily answered. The fact that the most refrangible of the three lines in the nebulae coincides in position with the line F, which is the second of the conspicuous lines of hydrogen, points to hydrogen as probably one of the gases present. This conclusion seems to be rendered almost certain by the circumstance that in one of the nebulae Dr Huggins was able to make out, in addition to the three bright lines characteristic of the nebulae in general, a fourth bright line considerably more refrangible, which coincided with the third

conspicuous line of hydrogen, that near G, as was also seen later in the nebula of Orion.

The fact that the bright line C in the red, which in a Geissler's tube is commonly the most conspicuous of the hydrogen lines, is absent from the spectra of the nebulae, might seem to throw a difficulty in the way of this conclusion. But Plücker and Hittorff found that when the gas is very highly attenuated the line C disappears; besides which it is to be borne in mind that when the intensity of light is greatly reduced, the light of lower refrangibility ceases to be perceived before that of higher.

There remain two of the nebular lines to account for. The least refrangible coincides with a line of nitrogen, or rather with one of its components, for the nitrogen line was generally seen double, the nebular line coinciding with the less refrangible component, while the line in the nebula always appeared single. Dr Huggins accordingly left it doubtful whether the first of the nebular lines ought properly to be referred to nitrogen, though later research seems to favour the supposition that it ought. The second line however at any rate still remains unaccounted for. It may possibly indicate some form of matter more elementary than any we know on earth.

There seems no *a priori* improbability in such a supposition so great as to lead us at once to reject

it. Chemists have long speculated on the so-called elements, or many of them, being merely very stable compounds of elements of a higher order, or even perhaps of a single kind of matter; and the combination of observations of the sun's surface with experiments in the laboratory have been thought by some to be favourable to such a view.

Another example of the information obtained by recent methods of research as to what is going on in distant parts of the universe is afforded by an event which took place in the year 1866.

On more than one occasion it is recorded in history that a new star burst forth in the heavens, and after remaining brilliant for a considerable time gradually faded away. One such star appeared in the time of the Greek astronomer Hipparchus. Another was seen in the time of Tycho Brahe. The latter was as bright as one of the principal planets, and remained visible for many months before it faded away, and that so completely that now no star appears in its place.

Now a somewhat similar phenomenon was seen in 1866, though on a much smaller scale both as to magnitude and duration. A star appeared in the constellation of the Northern Crown which observers who had been so in the habit of watching the heavens that they had as I may say got the stars by heart

recognised as new. Of course the discovery was immediately published, and astronomers directed their instruments to the new comer. It was none too soon, for the brightness of the star waned rapidly, so that even one or two days' delay made a difference. At the brightest, the star did not near equal the one which appeared in the time of Tycho Brahe; but not to mention the improvement in telescopes, astronomers are now in possession of an instrument unthought of in his days; I allude to a star spectroscope. When the new star was examined by the spectroscope, the spectrum was found to be of a composite character, resembling the ordinary spectrum of a star with a bright-line spectrum superposed on it. This indicated that a good part of the light coming from the star was due to incandescent gas; and Dr Huggins mentions that as the star faded, the continuous spectrum became relatively feebler than the spectrum which consisted of bright lines. In the bright-line spectrum the indications of hydrogen were as usual conspicuous.

It does not seem probable that such a unique event in the history of a star is merely an exaggeration of the regularly recurring phenomena of a change of brightness in the so-called variable stars, more especially as spectroscopic examination has not hitherto revealed the existence of a bright-line spectrum in the light of the variable stars. It

would rather seem to point to some tremendous conflagration, whether due to the collision of two bodies in the interstellar spaces, or to some other cause. The distance to which the glowing gas extended must have been enormous to have subtended a sensible angle at the earth, giving in the telescope the appearance of a misty star. What may haye been the date of the actual occurrence which we witnessed in 1866 we do not know. It must have been several years earlier, as light would take years to travel from the star to us.

In contemplating so gigantic a catastrophe, our thoughts can hardly fail to turn to what we are led to expect as the ultimate fate of our earth.

There is another strange class of heavenly bodies, belonging to our own solar system, in the elucidation of the nature of which the properties of light have been of much avail; I allude to comets. Briefly to refer to what will be found in elementary books, I may say that they move in orbits which are elliptic and highly eccentric, with the sun in one focus of the ellipse, or commonly in orbits which are sensibly parabolic for that part of them in traversing which the comet can alone be seen. It is said that some comets have been ascertained to move in hyperbolic orbits, in which case they must have been temporary visitants to our system, or at least on leaving the sun

have gone off into space. In either case the comet is comparatively near the sun for a limited part only of its orbit. In approaching the sun from a distance, it at first appears like a misty star, but as it gets nearer a tail appears, which rapidly increases in length as the comet approaches its perihelion, or point of its orbit nearest to the sun, after passing which the tail is commonly even better developed than at an equal distance before perihelion. It is remarkable that at least with rare exceptions the tail is turned away from the sun, whether the comet is approaching perihelion or receding from it. It is also curved, the convexity being on the side towards which the comet is moving.

Now what notion can we form of the nature of that strange appendage, the tail, darted forth from the comet, if darted forth it be, with a velocity sometimes so enormous, whisked round, if whisked round it be, as the comet passes perihelion, with a velocity in a direction perpendicular to the radius vector drawn to the sun which in the outer portions of the tail must immensely exceed that of the comet itself?

To evade these difficulties, some have broached theories according to which the tail would not consist of matter derived from the comet, that is, from its nucleus, but would arise from some change of condition, or difference in the mode of illumination, of

matter there already. But among astronomers, who were familiar with the appearances of the heads of comets as seen in good telescopes, the opinion I think was generally entertained that a portion at least of the envelope and tail consisted of matter ejected from the nucleus.

Let us now see what evidence can be brought to bear on the question by an examination of the light of the comet.

If the tail were self-luminous, its light ought to show no polarization. If it were due, in part at least, to the reflection of light from the sun falling on dust or condensed vapour, it ought to show more or less distinct signs of polarization. The fact is that the light of the tail shows very decided traces of polarization, as has long been known.

Of recent years the light of comets has been examined spectroscopically, by Dr Huggins and others. The result is very remarkable. The nucleus and a small portion of the root of the tail are found to show a spectrum with bright bands, which are mainly the same in the greater number of comets, though some show peculiarities of their own, and which curiously enough agree with the bright bands shown in the slightly luminous flames of hydrocarbons, such for example as the blue base of the flame of a candle.

There can no longer be any doubt that the nucleus consists, in its inner portions at least, of vapour of some kind, and we must now add incandescent vapour; nor does there appear to be any reasonable doubt that in most comets this vapour consists of or contains some volatile compound of carbon, unless it be carbon itself vaporized by the heat of the sun. Whether we are to attribute the bright bands to a compound of carbon or to carbon itself, is a point which has been a good deal debated, and into which I forbear to enter, though I cannot help entertaining an opinion.

To account for the telescopic appearance of the head and envelope, we must I think with Sir John Herschel admit the existence of a repulsive force emanating from the sun, a force perhaps exerted, not on the incandescent vapour itself, but on the highly tenuous cloud resulting from its condensation. According to the view which I would here present to you, the tail consists of mist of exceeding tenuity continually streaming away from the head under the influence of the repulsive force and passing into space, and continually renewed by condensation of fresh vapour which is continually ejected from the nucleus, so long as the comet is sufficiently near the sun. There is no whisking round of the tail as the comet moves round near perihelion, because the tail,

though material, does not consist of the same matter, but of matter which is continually renewed.

Suppose a fire engine and a fireman with a hose mounted on a platform which is capable of revolving round a vertical axis, and suppose the man always to direct the hose so as to send a stream outwards from the axis when the platform is at rest. Let the platform now be moved round. Then the outward flowing stream will be curved with its convexity foremost, not on account of the resistance of the air, or at least very little on that account, but because the velocity of a particle of water in a direction perpendicular to the radius vector is such as to allow equal areas to be swept by the radius vector in equal times, and the angular velocity of the radius vector becomes less and less as the particle goes outwards.

The fan-shaped tails which comets sometimes exhibit is easily explained on this view by the occurrence of different jets of vapour from the nucleus, which harmonises very well with the telescopic appearances. If the velocity of ejection be not very small compared with the velocity of the nucleus in its orbit, the velocity and direction of motion of the vapour which parts company with the nucleus will be sensibly different from that of the nucleus, and the portion of the tail which is due to such a jet will be modified accordingly.

This theory so far leaves two things unexplained—the source of the very high temperature at such a distance from the sun required to render the vapour self-luminous, and that of the repulsive force.

As to the former, it may be that the emissive powers of bodies, until very high temperatures are reached, have been much overrated, from the circumstance that the experiments from which the conclusions were deduced were made in air or other gas, either at ordinary pressures or at rarefactions short of those very high ones which are now reached. Thus Kundt found that the rate of cooling of a heated wire in a vessel filled with air or other gas, when examined at different pressures, decreased indeed as the exhaustion proceeded, but at a very moderate exhaustion became sensibly constant, and remained so up to the highest exhaustion he was able to obtain. He naturally concluded that he had reached the rate due to radiation alone. But Mr Crookes, whose skill is so great in producing excessive exhaustions, while he verified Kundt's result up to very high exhaustions, found that on going still further onwards the rate of cooling began rapidly to diminish, nor with all his skill in exhaustion did he succeed in getting to a second limit where it remained constant again, this time doubtless at the rate which would be due to radiation alone. Now the planetary vacua probably far surpass

what we can produce in the laboratory, and it may be that up to a very high temperature the emission of radiant heat from a small body like the nucleus of a comet, with perhaps no atmosphere but what was produced by evaporation and was passing off into space, is a good deal less than we should have supposed from laboratory experiments.

But perhaps a more probable view is what may be called the green-house theory. The explanation of the warmth of a green-house as is well known depends on the different behaviour of the radiant heat from the sun and of that which comes from the plants, stands, etc., in the green-house, with reference to their passage through glass. The former is mainly heat of high refrangibility, which passes freely through glass; the latter is mainly heat of low refrangibility, with respect to which glass is opaque. Accordingly the house gets warmed much above the temperature it would have if glass had the diathermanous properties of rock salt. It is even said that water may be boiled by putting it in sunshine under a series of glass enclosures.

Now it is conceivable that if the nucleus of a comet be endowed with an atmosphere, or perhaps even coated with a liquid, having in a high degree the combination of the transparent and athermanous characters of glass, its temperature when exposed to

radiation from the sun might rise much above what we might have expected *a priori*.

As to the supposed repulsive force, Sir John Herschel in his Cape Observations has thrown out a conjecture to which recent researches have contributed increased probability. He suggests that the sun may possibly be a permanently charged electrified body, and that the condensed vapour from the nucleus, or at any rate belonging to the comet, may be charged with electricity of the same name, and may therefore be repelled. Now Mr Crookes found that in one of his highest vacua a pair of very small gold leaves made to diverge by a small charge of electricity remained diverging, apparently to the same extent, for a whole year. When we reflect what a very small charge of electricity such a pair of gold leaves could hold, we are led to the inference that a perfect vacuum, that is, a space containing no *ponderable* matter, would be a perfect insulator. There is nothing therefore unreasonable in supposing that the sun may be a permanently charged body.

On the other hand the well-known connexion of cumulus clouds with thunderstorms, and the evident formation of cumuli from the precipitation of vapour consequent on the cold produced by the expansion of ascending columns of heated air charged with moisture, seem to leave little doubt that in our atmos-

phere rapid condensation of vapour is as a matter of fact connected with high electric manifestations, whatever may be the cause of the connexion. It is no violent supposition therefore to make, to suppose that the condensation of the vapour coming from the nucleus of a comet may cause a similar development of electricity.

Of course if matter from a comet is thus driven off into space, the comet must gradually waste away, however slowly, so far at least as such portion of it as consists of matter thus vaporizable and driven off into the tail is concerned, after which the residue might be compared to the coke in a gas retort; and as it no longer presents the large volume due to the volatilized vapour, it would remain invisible from its extreme smallness. A very small quantity of matter might suffice for the display at each revolution, and besides we do not know how long a comet has been appearing as such. It may have been circulating for ages round the sun a long way off, until at last it passed, casually as we might say, near a planet, the attraction of which gave it a new orbit altogether, and brought it within roasting distance of the sun when near its perihelion, when it assumed its cometary character. This theory would account for the great eccentricity of the orbits of comets; for the perturbation produced by a distant planet, such as Uranus or Neptune, to

which in circulating round the sun it at last made a near approach would tell far more on the direction of motion than on the velocity, so that when the body got near the sun, its velocity would usually be a little, and only a little, less than that in a parabolic orbit.

LECTURE IV.

Red prominences seen about the sun in total eclipses—Inferences as to their character derived from an examination of their light—Mode of viewing them independently of an eclipse—Evidence they afford of gigantic commotion—Corona—Alteration in the pitch of sound produced by motion of the source of sound, or of the observer—Analogous alteration of the refrangibility of light—Indications thus afforded of motions of approach or recess of the stars relatively to the earth—Indications of commotion in the sun—Application to the discrimination between dark lines of solar and of terrestrial origin in the solar spectrum—Views which we are led to entertain as to the constitution and history of the sun and stars—Conclusion.

IN the applications of the spectroscope to the heavenly bodies which I have hitherto brought before you, with the exception of those relating to the spectrum of the sun taken as a whole, one difficulty to contend with has been the feebleness of the light. I come now to a research where the difficulty has in so

far been in the opposite direction as that it was due to the excess of light mixed up with that which it was desired to observe.

The astronomers who observed the total solar eclipse of 1842, which was visible in Italy, noticed particularly a phenomenon which they had not at all expected. They were fully prepared to observe the corona, but they were surprised to observe in addition to this three or four rose-coloured prominences like luminous mountains surrounding the dark disc of the moon. The duration of the total phase in this eclipse being rather short, and the phenomenon unexpected, naturally not much could be done on that occasion towards investigating their nature. But a highly interesting subject for study in future eclipses of the sun was pointed out.

With reference to the nature of the prominences, one important point to decide of course was whether they belonged to the sun or the moon. The decision of this was not difficult, for if they belonged to the moon they would retain the same positions relative to the lunar disc all through totality, whereas if they belonged to the sun they would be covered or uncovered, as the case might be, by the advancing moon. The latter proved to be the case.

In the total eclipse of 1851 a daguerreotype of the prominences was taken by Dr Busch, but it does not

appear to have been good. In 1860 a total eclipse was to be visible in Spain, and the Admiralty lent a vessel to convey the observers. Among them, Mr De La Rue, who had been so successful in celestial photography, went prepared to take a photograph of the totality. Not knowing what might be the actinic power of the prominences, he thought it safest to give the whole time of totality, which was under two minutes, to one photograph, or rather pair of photographs, lest in aiming at catching two or more phases the time should be too short to impress the plates. The event proved that a much shorter time of exposure would have sufficed; for the actinic power proved to be very great, a conclusion in itself of much importance at the time. The strength of the actinic power was shown among other ways by the pictures exhibiting a prominence in the form of a detached cloud, which if not invisible to the eye had at any rate not been noticed by the eye-observers during the brief time they had to scrutinise the phenomenon.

Another observation of importance at the time as bearing on the nature of the prominences is due to M. Prazmouski, who had made well-devised preparations for determining the polarization, if any, of the prominences and corona separately. He found that the corona was strongly polarized in a radial direction, though not so strongly near the sun as a little

way off. It follows that the light of the corona must, in part at least, be reflected light, and that its real seat must be round the sun, not round the moon. The light of the prominences on the other hand was altogether unpolarized. The most natural conclusion would seem to have been that they were self-luminous, though Prazmouski drew another, which does not seem to have been altogether sustainable.

Total solar eclipses do not occur very frequently, perhaps on an average once in two years. Unlike eclipses of the moon, the total phase is visible only along a narrow strip of the earth's surface, perhaps 100 miles broad or less, and this strip may be on the ocean, where observers have no firm footing for their instruments, or if not that it may be in some distant and out-of-the-way part of the earth. And even if observers make a long journey on purpose, taking their chance for absence of cloud at the critical moment, or if the total phase happens to be visible nearer home, there is only a very short time, seldom exceeding four minutes, in which to make all the observations which demand totality. Hence so long as we are confined to total solar eclipses for our knowledge of the prominences, our progress in the study of their nature must necessarily be slow.

Mr Lockyer had for some time been engaged in solar study, examining more particularly the spots

with a spectroscope, when he published the suggestion that possibly the spectroscope might enable us to detect the presence of the prominences independently of a total eclipse of the sun. Such a result might be hoped for if their spectrum, which had not then been examined, should give bright lines. The reason of this is easily understood. If we double, treble, &c. the dispersion of a spectroscope, the width of the slit remaining the same, the same quantity of light as before goes to form a given portion of a continuous spectrum. But this is of two, three... times the length it was before, and therefore the brightness is only one-half, one third... as great as at first. But if besides the continuous spectrum there be a source of strictly monochromatic light, the image of the slit as seen by this, forming what we call a bright line in the spectrum, is not more spread out by the increase of dispersion, and therefore as we increase the dispersion, the bright line continually gains in brightness relatively to the continuous spectrum. Now in attempting to make out the presence of prominences by the bright lines of their spectra, if such they have, we encounter a continuous spectrum due, partly to the solar corona, partly to the light dispersed in our own atmosphere, light which we all know is very bright in the immediate neighbourhood of the sun.

Mr Lockyer tried repeatedly to detect the promi-

nences with his instrument, but failed. Another spectroscope of greater power was then ordered. Almost immediately after this was received, success crowned his efforts. On the 19th of November 1868 he succeeded by this method in detecting a prominence by the occurrence of the bright line C in the spectrum of light taken from immediately outside the sun in one part of his disc. Shortly afterwards a prominence was detected by the bright line F as well as C. An account of this discovery was communicated next day to the Royal Society.

Meanwhile M. Janssen had gone out to India to observe a total eclipse to be visible there on the 18th of August of the same year. He went specially prepared to examine the spectrum of the prominences. He was favoured with clear weather, and found that the spectrum in question was a bright-line one, indicating the presence of incandescent gas. The observation seemed to him so easy that it occurred to him that it might be possible by the application of the same method to make out prominences on any day if there were any present; and accordingly next day he tried the thing and succeeded. This result was not communicated by telegraph, nor did the account of it reach Europe until after Mr Lockyer's discovery had been communicated to the Royal Society and to the French Academy.

In this mode of examining the prominences, the prominence can only be made out piecemeal, by taking different sections of it by the slit of the spectroscope, observing the portion of the slit to which the bright line as seen in the spectroscope corresponds, and then putting all the sections together. Shortly after the announcement of Mr Lockyer's discovery, Dr Huggins modified the method by widening the slit sufficiently to take in a whole prominence, and preventing the drowning of the prominence by the light thus let in by using a suitable absorbing medium; actually by the use of a red glass. This method, or else that of the narrow slit, is now used habitually, and daily observations, weather permitting, of the prominences are now regularly made.

Of course total eclipses still afford the best opportunity for scrutinizing the spectra of prominences. They are found, especially in the lower parts, to show bright lines due to several metals, but the hydrogen lines are the most characteristic, and it is by means of the hydrogen line C in the red, or else by F in the blue, that the forms of the prominences are made out on ordinary days.

One of the most astounding things connected with these prominences, now that we are able to study them at any time, is the enormous rate at which they are developed. They are shot forth from

the body of the sun with velocities going sometimes up to 100 miles per second, or even beyond. Their most usual forms give the idea of the ejection of a gas, which gradually is brought down again by gravitation towards the surface of the sun.

We appear to know less about the corona than about the prominences. At any rate till quite lately, we were dependent for all our knowledge about it on such observations as could be taken during the brief duration of total eclipses, in which there were other things besides the corona that claimed attention. I said till quite lately, because now Dr Huggins appears to have succeeded in obtaining photographically some representation of it independently of eclipses. The great difficulty was of course to eliminate it from the glare due to our own atmosphere that is always seen close to the sun. The preliminary trials, when the pictures were carefully examined, seemed to leave little doubt that the corona was really depicted on the photographs, though of course on account of atmospheric glare the pictures must be much inferior to what can be got during total solar eclipses. The corona has to be picked out, as best may be, from the effect of the glare by delicate differences of intensity. Of course the great object to aim at in these photographs is to obtain if possible a continuous history of the changes which may take place in the form of the

corona; for though the pictures which can be taken during totality in an eclipse are far superior for the form of the corona at the moment, the occasions on which they can be taken are too rare to furnish us with much of a history.

As to the character of the light of the corona, its polarization as already mentioned shows that the light is in part at least reflected. Spectroscopic observation shows, besides a few bright lines in the part near the sun, a peculiar line agreeing in position with the brightest line of the aurora, and extending higher up. This shows that the corona is to some extent self-luminous, especially near the sun, which falls in with Prazmouski's observation that the polarization appeared stronger a little way from the sun than close to the disc. There is however a circumstance independent of self-luminosity which would tend towards the same result. What the particular substance or condition of a substance may be that gives rise to the green line common to the corona and the aurora, requires further elucidation.

In my course of lectures last year I frequently referred to the phenomena of sound as illustrating, or suggesting the explanation of, certain phenomena of light. In the present year's course I have not hitherto referred to sound; but I now come to a phenomenon regarding light for an easier apprehension of

which it will be convenient to consider the analogous phenomenon in sound.

Suppose that a person is standing on the platform of a railway station when an express train passes by at full speed, and that the engine driver keeps the whistle sounding as the train rushes past. If the observer on the platform notices the pitch of the whistle, he will find that there is a very sensible fall of pitch when the train has passed. The same thing would be observed still more strikingly when two trains moving in opposite directions pass each other at full speed, the observer being in one, and the engine driver of the other keeping the whistle sounding as the trains pass.

Now the whistle is the same all along, and worked in the same way. What then is the cause of the lowering of pitch?

The pitch of a musical note depends we know on the frequency of the vibrations, on the number, that is, of pulses per second which strike the ear. When that number is doubled, the pitch is an octave higher, when halved, an octave lower, and the ratio for other musical intervals is well known. Suppose now that a sustained series of vibrations proceeds from a source of sound *W*, and the sound is heard by an observer *O*, the air being quite calm at the time, and the source *W* and the observer *O* being at rest. Then

it is easily seen that the number of pulses per second which emanate from *W* is just the same as the number which strike the ear of the observer; the effect of the finite velocity of propagation of sound is merely that the individual pulses emanating from *W* do not affect the observer until the expiration of the time that sound takes to travel from *W* to *O*.

If the wind be blowing, the result is still the same, as we see at once if we suppose the vibrations to go on indefinitely, since just as many vibrations as start from *W* must reach *O*. The pitch therefore of the note is not altered by the wind.

But it is otherwise if the source *W* be moving towards or from the observer. Suppose that in any given short time *W* moves to *W'*, in the line *WO*, in proceeding towards *O* with the velocity *v*, and let *V* be the velocity of sound in still air. As the source travels from *W* to *W'*, it is continually sending out vibrations at the normal number per second corresponding to the pitch of the whistle or other source, and these travel towards *O* with the velocity of sound. Had all the pulses started from *W'* at their due intervals of succession, they would have been received at *O* at the same intervals apart; but as it is, those that start from the source on its journey to *W'* are handicapped by the time sound takes to travel to *W'*. Hence whereas the whole time occupied by the issue

of the series of vibrations is that the source takes to travel from W to W' with velocity v, the whole time occupied in the reception of the vibrations at O is less than the former by the time sound takes to travel from W to W' with the velocity V. Hence the ratio of the durations of issue and reception of the same series of vibrations is that of V to $V - v$; and since the ratio of the frequencies, or numbers of pulses per second, is the inverse of the former, the frequency of reception will be to the frequency of issue as V to $V - v$. The pitch of the note as heard at O will accordingly be raised by the motion of the source towards O by the quantity corresponding to the above ratio.

In a precisely similar way, if the source be receding from O instead of approaching it with the velocity v, the frequency of reception will be to the frequency of issue as V to $V + v$, and the pitch of the note will be lowered accordingly. And if the source be first approaching to and afterwards receding from O with the velocity v, the ratio of the frequencies as the source approaches and recedes will be that of $V + v$ to $V - v$.

Thus supposing the source to be the whistle of a train travelling at the rate of 45 miles an hour or 66 feet per second, and taking 1100 feet per second for the velocity of sound, we have for the ratio of the frequencies in approach and recess 1166 to 1034, or

1128 to 1000, nearly. This slightly exceeds the ratio, 9 to 8, or 1125 to 1000, which is that for a major tone. The effect therefore is quite palpable, and would be easily perceived even though the train were travelling a good deal more slowly.

If instead of the source moving towards the observer, the observer is moving towards the source with the velocity v', the source itself being at rest, a little consideration will show that the effect is the very same, and the ratio of the frequency of the sound heard to the frequency of issue is that of V to $V - v'$; and if the source and the observer be both moving towards each other with velocities v, v' respectively, the ratio of the frequency as heard and as issued will be that of V to $V - (v + v')$. This does not necessarily suppose that v and v' are very small compared with V; and the formula will apply to all cases whether of approach or recess, if we suppose v or v' to be negative when the source or the observer is receding instead of approaching.

Now if light consists in undulations, the same thing ought to occur with it. The velocity of light, about 186000 miles per second, is however so enormous, that any velocity we can mechanically produce on earth is too insignificant in comparison sensibly to affect the frequency of the vibrations reaching us, or consequently the refrangibility, which depends on the

frequency. But in the earth and heavenly bodies we have masses moving with velocities which are not incomparably smaller than the velocity of light. It is conceivable therefore that some indication of a variation of frequency should be capable of detection in connexion with these motions. Thus the earth in its motion round the sun moves in round numbers 20 miles in a second, and accurate measurements of the positions of the stars show that many of them have what is called proper motions; that is, individual stars show small progressive changes of position relative to the body of stars taken as a whole. And since on the whole there appears to be a slight opening out of the stars on one side of the heavens, especially in the neighbourhood of the constellation Hercules, and a closing in on the opposite side, it has been concluded that the whole solar system is probably moving in space in the direction of that constellation.

The only motion of the earth and the stars of which we can thus take cognizance is that part of the relative motion of the earth and any particular star which is transverse to the line joining them. And as the displacements observed are all angular displacements, displacements, that is, of the direction of the joining line, we can only draw a conclusion as to the relative velocities in a transverse direction on condi-

tion that we know the distances of the stars. The distances of the stars are so enormous that it is only in some cases that astronomers have been able to determine them roughly with some degree of confidence, through examination of the annual parallax. It has been concluded as probable that our own system is moving relatively to the stars as a whole with a velocity something like that of the earth in its orbit round the sun.

Inasmuch as in contemplating the stellar universe our own sun is merely one particular star, taken at random, there is no reason why the direction of motion of a star in space should have any particular relation to that of the line joining the star with the sun. If then motions in a direction perpendicular to the joining line be revealed by astronomical observation, the probability is that motions also exist in the direction of the joining line, though of these no cognizance can be taken by observations of the positions of the stars; and if velocities of approach or recess of the stars relatively to the sun exist, it may be that they might admit of detection by the change of refrangibility which they would occasion.

In the illustration I gave of the railway whistle, the effect of the motion of the train on the pitch of the sound as heard by the observer was detected by the sudden change from a higher to a lower pitch as

the train passed by. We have no such means available in the case of a star. How then would a change of refrangibility, if it exists, admit of detection?

To go back to the railway, suppose the driver of the approaching train were to shut off the steam from his whistle before he reached the station; how in that case could the observer on the platform detect a change in the pitch of the whistle as heard by him from what it would have been if the train had been at rest? Only by comparing the note given by a whistle, or similar instrument which he carried about him, and which had previously been adjusted to unison with the same railway whistle when the train was at rest, with the note heard when the train is approaching the station.

Now can we find two kinds of light, of definite refrangibility, one on earth and one in a star, which we know to be of equal frequency at issue?

I have already referred to the remarkable spectrum of Sirius, and the overwhelming evidence which the coincidences of so many of its dark lines with the bright lines in the spectrum of incandescent hydrogen afford of the existence of hydrogen in the star. To this substance would correspond definite frequencies of vibration whether it vibrated on earth or in Sirius, assuming of course that the same laws of matter exist in that distant star as on earth. But if the

frequencies of issue were the same, the frequencies of the light as received on earth ought not to be rigorously the same, if the relative velocity of approach or recess of the earth and Sirius be not insensibly small compared with the velocity of light.

Now on comparing with all possible accuracy the positions of the dark line F in the spectrum of Sirius with the bright line F in incandescent hydrogen, Dr Huggins found that the coincidence was not quite perfect; the centre of the bright line F lay towards the more refrangible edge of the dark line, or rather very narrow band, in the spectrum of Sirius.

Now assuming that the frequency of issue was the same in the two cases, such a result would be produced by a suitable velocity of recess, and we might deduce that velocity from a measurement of the amount of displacement. The displacement is so excessively small that in any attempt at measurement large allowance must be made for the inevitable errors of observation. Still a tolerably satisfactory result was arrived at, and it appeared that at the time of the first series of observations the relative velocity of recess of Sirius from the earth was about 41 miles per second. Deducting from this the portion due to the motion of the earth in its orbit, there remained about 29 miles per second as the velocity of recess of Sirius relatively to the sun.

The path thus opened out has been followed up by Dr Huggins himself and by others; and comparisons between the dark lines of stars and artificial bright lines, with a view to determine the longitudinal components of the motions of the stars relatively to our sun, are now regularly carried on at the Royal Observatory, Greenwich.

It may appear at first sight illogical to draw conclusions as to the existence of such or such an element in a star from the coincidence of certain lines in its spectrum with bright lines in artificial sources, and then to draw further conclusions from the imperfection of the coincidence. This however is not so. If we could affirm that the coincidence ought to be mathematically perfect on the supposition that the substance in the star was the same as what we have on earth, then indeed a defect of coincidence would be fatal. But if on the contrary we can assign a cause why the coincidence should not be perfect, especially if the conditions assigned for the non-coincidence are far more likely to be present than not, then the case is very different. Then, with one important exception, the evidence for the existence of the substance in the star is the same as it would be if the coincidence were observed by a less perfect instrument in which the errors of observation would prevent us from being able to affirm with the same

exactitude as with the better instrument that there was or was not a coincidence; but the less exact instrument might still leave the coincidences far too striking to be attributable to chance.

The exception referred to is, that the explanation of non-coincidence by relative motion leaves but one disposable constant whereby to account for the defects of coincidence, so that there ought to be a relation between the defects in the different parts of the spectrum, following an assignable law. No such discrepancy between the results obtained from different parts of the spectrum as would lead us to reject the explanation of imperfection of coincidence by proper motion has hitherto been shown to exist.

The alteration of refrangibility due to relative motion has been observed on a body nearer home—on our own sun. Mr Lockyer has specially studied the behaviour of spots and their neighbourhood with respect to spectral lines, and has found instances in which bright lines, such as the C or F line of hydrogen, have actually been seen on the disc of the sun. Moreover the lines in spots and their neighbourhood, whether bright or dark, are sometimes much contorted, indicating extraordinarily violent up or down motions of the gases. A velocity as great as 140 miles per second has thus been observed through the attendant change of refrangibility. The general

phenomena seem to indicate that the bright lines belong to portions of intensely heated gas which have rushed up from the interior of the sun, from what depth of course there is no way of saying, while the spots are due to the absorption of light from below by cooler, though still intensely hot, gases which are commonly descending.

Before I conclude I must briefly refer to a singularly elegant method which M. Cornu has devised for distinguishing between those dark lines of the solar spectrum which owe their origin to the sun and those which are due to absorption of the sun's light by the atmosphere of the earth. He places a lens mounted so as to admit of a rapid lateral oscillating motion in front of the slit of the spectroscope, which for this purpose must be one of very high dispersion, at such a distance as to form an image of the sun on the slit. The amount of lateral motion is such as to bring the two lateral limbs of the sun, near the edges, so as to fall on the slit alternately. The telescope is so mounted that the slit is, roughly at least, perpendicular to the sun's equator.

Now in consequence of the sun's rotation, from west to east, round an axis, his eastern limb is moving towards us, and the western limb from us, with a velocity of about 1¼ mile per second. The difference of refrangibility between the two for light

of a given frequency of issue would be that corresponding to a velocity of approach or of recess double of the above, and the corresponding displacement would not be insensible in a spectroscope of very high dispersion. Now when the lens is made to oscillate, the eastern and western limbs are brought on the slit alternately in quick succession, and the consequence is that there is a slight lateral oscillation in the lines which are of solar origin. But there is no difference made as regards the lines that are due to absorption by the earth's atmosphere, since as regards them the solar light is tantamount to light furnishing a continuous spectrum, and the absorption takes place for light of a given frequency of reception, not frequency of issue. Accordingly the lines of solar and those of terrestrial origin are distinguished at once, simply by looking at them, the former oscillating a little right and left, while the latter remain fixed.

Let us pause a little here to consider the views which our present knowledge opens out to us as to the constitution of the sun.

We have seen that the interpretation of the dark lines of the solar spectrum leads us to regard even the outer portions of the sun's atmosphere as still intensely hot, so much so that even such refractory substances as iron, chromium, &c. are kept in a state

of vapour. If such a temperature be maintained, notwithstanding the enormous and constant loss of heat from the sun by radiation into space, the body of the sun must be hotter still. It must therefore be at a temperature great enough to keep iron, chromium, &c. in a state of vapour, unless it be that they are liquefied by pressure notwithstanding the higher temperature. Dr Andrews has shown that for such liquids as liquefied carbonic acid, ether, &c., and presumably for all liquids, there is a critical temperature, differing from liquid to liquid, above which there is a perfectly continuous passage, as the pressure is increased, from what everybody would call gas to what everybody would call liquid. It may be that the temperature of the sun is above the critical point of iron and other known elements which are ordinarily considered highly refractory. It may be that at the intense temperature of the body of the sun iron is dissociated, and resolved into still more elementary forms of matter, while in the neighbourhood of the surface it is re-formed by the combination of elements of a higher order. The results of constant observations on the iron lines of the solar spectrum under varying conditions such as spots, date of observation, &c., and a comparison of these results with those relating to the bright lines of iron produced by different forms of the electric dis-

charge, have led Mr Lockyer to be in favour of such a view. But as regards the maintenance of the energy which is continually lost from the surface of the sun by radiation into space, it signifies little whether we suppose the transition from the vapour of iron in the superficial parts of the sun to molten iron at a higher temperature in the parts lying more deeply to be continuous or abrupt, or again whether we suppose that iron as such, whether in the state of vapour or as one constituent of a molten mass, exists throughout, or only in the parts near the surface, being formed from more elementary kinds of matter in the deeper, hotter parts. The same applies to the other substances which we call elements. On any of these suppositions the general process of maintenance would be the same; the intensely heated, outer portions which radiate into space must get somewhat cooled thereby; and presently the outer strata getting cooled as a whole by the accumulation of the effects of convection currents on a small scale, we have an interchange on a grand scale between the cooler strata above and more intensely heated matter from below. These uprushes of most intensely heated gas form the prominences which are traceable round the edge of the sun, and they are sometimes indicated even on the full disc by the exhibition in the spectroscope of lines which are seen as bright in spite of

the intense luminosity of the disc. The downrushes of the gases which though absolutely intensely hot are relatively cool, appear as dark spots or patches on the sun's surface, in consequence of their absorbing action on the light radiating from the hotter parts below, which they do not make up for by their own inferior radiation. These interchanges on the grand scale between the more and less intensely heated gases appear to be subject to a rough periodicity, the period being about 11 years. But interchanges on a smaller scale are always going on, analogous to the small convection currents in air which are always taking place near the surface of the earth when the sun is shining. With a sufficiently high magnifying power and sharpness of definition, the sun's surface is seen to be dotted over with minute black spots called pores, and between these are small brighter patches, the same apparently as the willow leaves of Nasmyth. These are admirably depicted in the large-scale photographs of M. Janssen; and so rapid are the changes that two photographs taken at an interval of even a few seconds are found not to be identical. So constant is the state of turmoil in the central luminary of our system, which is like a huge boiling pot, nearly a million of miles in diameter, only that the currents of convection arise from cooling at the top instead of heating at the

bottom; and were it not for that, did the interchanges take place only at considerable intervals, the light and heat of the sun would be subject to extensive and fitful changes, prejudicial to the welfare of the living beings which inhabit our earth, and it may be other planets of our system. Thus everywhere we behold that mutual adaptation of the various parts of the system of nature which leads us to the conception of a designing mind.

The radiation from the sun is the proximate maintainer of processes which are essential to life, whether animal or vegetable, as I hope to explain more fully in my concluding course of lectures; and as regards this the most recent discoveries teach us more and more that we are living upon capital. The sun is a vast bank of energy, but the supply, though it may last for ages, is not inexhaustible. Progress, not periodicity, appears to be the order of things when we contemplate them on the great scale. The present order of physical nature cannot be pushed back to a past eternity, nor is it calculated to remain as it is for an eternity to come. It may be that in the planetary nebulae we are permitted to witness an early stage in the process of creation. Even when stellar points exist in it, we can hardly suppose that the system is adapted to the habitation of living organized beings. Bathed in a fiery gas, of fiercer

temperature than the oxyhydrogen jet, though that melts platinum like sealing wax in a candle, the system could not support life at all analogous to what we have on earth, nor would it be adapted for such a purpose for ages to come. In the red stars on the other hand, with their peculiar spectra, it may be that we have systems which are either already effete, or which are in process of becoming so. The indications of the history of the universe which modern science has disclosed lead us to the contemplation of durations of time as vast in their way as are those distances in space with which we have long been familiar, from the result of astronomical measurements as applied to the solar system and the starry heavens.

The subject of my present course of lectures has carried our contemplations to the boundaries of the visible universe, and has given us fresh evidence of a similarity of plan running throughout, combined with individual difference of feature. The difference between the spectra of different stars is analogous, as Dr Huggins and Dr Miller have remarked, to the difference of materials between one locality on the earth and another. Just as in certain places some particular chemical element is found in quantity, though in general it may be rare, so in some of the stars we have abundant evidence of the presence of elements which in the earth, at least in the portions

close to the surface, which alone are accessible, and apparently in the sun too, occur but sparingly. We can hardly suppose that life is confined to one particular planet circulating around one particular sun out of this vast multitude. In face of the views that thus open out to us, the feeling of the littleness of man comes upon us with almost overwhelming force, so much has modern research emphasised those words uttered of old, "What is man that thou art mindful of him?" But when from the contemplation of such immeasurable distances we turn to an individual living being, when for example we consider the structure of our own bodies, and the wonderful adaptation of the various organs to their purpose, we see that the vastness of the universe has not caused the Creator to be unmindful of the least of his creatures. On some of those adaptations, in which light is concerned, I hope to dwell in my remaining course. It is rather on the vastness of the scale and yet unity of plan of the universe that this year's course has led us to ponder. And to pass from material construction to moral government, we are warned by the contemplation of material nature to expect a moral government on a vast scale, and carried out in obedience to general laws, which if we disregard we must take the consequences.

LECTURES ON LIGHT.

THIRD COURSE.

ON THE BENEFICIAL EFFECTS OF LIGHT.

LECTURE I.

Extended signification in which the word "light" will here be used—Different effects produced by light—Mechanical value of sunlight—The existence of winds depends upon light—So also does that of clouds and rain, of streams and rivers—Effects of the radiation from bodies warmed by the sun.

AT the conclusion of the lectures which I had the honour of delivering in this place two years ago, I stated that in accordance with a scheme which had been communicated to and approved by the Burnett trustees, the subject of the entire series of lectures was to be Light, which in each year's course was to be treated from a different point of view; and that in the third year's course light was to be considered in

relation to its beneficial effects. Such accordingly is the subject of the present, the concluding, course.

We habitually consider light as an agent external to us which excites in us the sensation of vision. When we study the behaviour of this agent, we learn that it proceeds from some source, travelling with an extremely great, but still finite and measurable velocity, in a course which is straight so long as it lies in the same medium; that on arriving at the confines of this medium and of some other, it is in part reflected and in part refracted, according to definite laws, and so forth as regards other properties. We further find that this agent is not all of the same kind, but that there is some quality about it which admits of continuous variation from one portion to another, though it remains the same in the same portion throughout its whole course. This difference is made known to us through a difference of refrangibility, though not by that alone; and by suitable methods we are able to separate the portions of different refrangibilities from one another, and obtain a pure spectrum.

But we find that there are other effects which the influence emanating from the source is capable of producing besides that of exciting the sensation of vision. It raises the temperature of the body exposed to it, provided at least the body does not let the

portion which is not reflected pass freely through; in some bodies it produces chemical changes; in some it excites phosphorescence. These effects might conceivably either be attributed to different agents mixed together in the radiation, or be regarded as different effects which one and the same agent was capable of producing.

If these effects were produced by different agents agreeing in the properties of reflection, refraction and dispersion, and accordingly mixed together even in the same part of a pure spectrum, we should expect that though not separable by refraction they yet would be so by other means, such for example as absorption; or at least that they would be so far separable as that we should be able to extinguish or weaken the agent of a given refrangibility producing one of these effects while that producing another is left in full vigour. If on the other hand the different effects were produced by one and the same agent, it would be impossible so to modify the influencing radiation as to suppress or weaken the agent producing one of these effects without at the same time suppressing or weakening the agent producing any of the others. It is needless to say that in putting these two alternatives to the test of actual experiment we must have regard to the different degrees of sensitiveness of the different effects regarded as indications of

the presence or absence of the agent to which they are respectively due.

Now when the experiment is actually made it is found that no matter what may be the treatment to which the influencing radiation may have been subjected, if one of the effects be prevented, so are the others, if one be weakened, so are the others. In accordance therefore with Newton's rule which forbids us needlessly to multiply the causes of natural phenomena, we are led to attribute these various effects to one and the same agent; being ready of course to modify our view should any phenomenon be discovered which would clearly show that it is possible to separate the agents by other means though not by refraction. And the view which our present knowledge leads us confidently to adopt respecting the nature of light—that it consists in undulations propagated through a medium—strengthens the probability, already so high, that these various effects are due to the same agent; for heat is now recognised as a mode of motion, and though we cannot explain in detail how it is that chemical changes and phosphorescence are brought about by the agency of ethereal vibrations, yet vibrations of the kind would seem to be very likely to produce such effects. In fact, the evidence which modern science affords us bearing on the question is such as to lead irresistibly to the con-

clusion that the different effects mentioned above as produced by something that radiates, are due to one and the same agent.

It follows that the presence of this agent may be evidenced by some of these effects even though others be absent. Thus suppose we use for the scrutiny of the incident radiations a solution of sulphate of quinine, and fix our attention on a particular part of the spectrum, say the neighbourhood of the line *F*. No fluorescence is produced, and no elevation of temperature. Yet radiation is there, and its presence is shown at once by receiving the light into the eye, either directly, or after being scattered by a screen; or again by the chemical action on a prepared photographic plate. Were we to classify the radiation belonging to the spectrum as a whole into that which did and that which did not produce fluorescence in the solution of quinine, the classification might be convenient in relation to a study of the salts of quinine, but considered purely in relation to the radiations themselves it would be wholly artificial and unmeaning.

Now we are in precisely the same condition if in studying the whole radiation we draw a distinction between that which affects and that which does not affect the eye. With reference to vision the distinction is all-important; with reference to the agent

considered in itself, considered objectively, the distinction is perfectly artificial and arbitrary. Beyond both ends of the visible spectrum there lie radiations which do not affect the eye, but are nevertheless, as we have every reason to believe, of the same physical nature as those which do, from which they do not differ by any inherent quality. As the agent which excites vision has been called from time immemorial light, or whatever may be the corresponding term in other languages, it will be convenient to use the same word to designate the agent considered in itself, and irrespectively of its capacity for exciting vision, a capacity which would be regarded as a mere accident of light, in the technical logical sense of that word. Accordingly I shall now use the word "light" to designate what for want of a better term I have just been calling radiation; a word which would more properly denote the process of radiation than the thing radiated, be it material or immaterial, be it matter or undulations.

The use of light in relation to vision is obvious to everybody. But it is only after science has made considerable progress that we learn that the very same agent, in fact light in the more extended sense in which I have now defined the term, has uses of still more vital importance in relation to our well-being, or I should rather say in relation to our existence at

all. Some persons are blind from birth, but yet lead tolerably happy lives, receiving it is true much assistance from their fellow creatures who are not subject to such a terrible deprivation. We can imagine ourselves accordingly living in a perfectly dark world, destitute of light of any kind, either natural or artificial, guiding ourselves as best might be by the exercise of the senses of touch and hearing. But the absence of light, of that agent of which the excitement of vision is only one of the offices, entails consequences extending far beyond even this.

Without a certain amount of warmth, men and animals cannot survive. Now what is the great source of warmth upon earth? Clearly the sun. We use indeed fires in our houses; but even if we could have had these fires independently of the sun, the heat thence derived, taking all the world over, is a mere nothing compared with the heat received directly from the sun. But the sun is separated from the earth by the enormous distance of 92 millions of miles, or thereabouts. How then is this supply of heat conveyed to us from such an enormous distance? It is as we familiarly know by radiation; it is therefore to light, in the extended sense in which the word is here used, that we owe this vast supply.

Although the power of the sun's heat is matter of common observation, we are hardly perhaps prepared

for the results obtained by an actual measurement of its amount. The quantity received at the earth's surface when the sun is shining clearly can be measured by suitable means, by observing the elevation of temperature produced in a known body which absorbs all the radiation. It must be noted however that the quantity we receive at the earth's surface is not as great as what falls on the earth as a whole, that is, inclusive of the atmosphere, since some portion is stopped in passing through the atmosphere, to allow for which a correction must be made.

Supposing the quantity which falls on a given area of the earth known, the quantity of heat which passes outwards into space in a given time through an equal area of the sun's surface can be got by multiplying by the ratio of the surface of a sphere described round the sun's centre and passing through the earth to the surface of the sun itself; and the mechanical equivalent of this amount of heat can be derived by means of Joule's equivalent, of 772 foot-pounds for the equivalent of the heat required to raise one pound of water through one degree of Fahrenheit's thermometer. About 30 years ago Sir William Thomson availed himself of data given by Pouillet for solar radiation to calculate the amount of energy actually emitted by the sun in the form of radiation. It appeared that for every square foot of the sun's

surface the energy of radiation was upwards of 6000 horse power. Yet even this prodigious quantity does not appear to come up to the actual amount; for Professor Langley in America by observations of solar radiation at low and high levels made by a new and delicate instrument of his own devising called a bolometer, which permitted him to operate separately on the different components of the total radiation after they had been separated by a prism, has found that the amount of absorption and scattering which takes place in the earth's atmosphere is greater than had been supposed, and his results lead us nearly to double the above estimate. We may take it then that the amount of energy poured forth into space corresponds to, in round numbers, 12,000 horse power per square foot. When we remember that the sun is a vast globe of about 855,000 miles in diameter, every square foot of the surface of which supplies energy at the above rate, and that that is continually going on from age to age, we cannot help feeling what a prodigious supply the sun must contain.

Of this radiant energy the earth, it is true, receives but a very small fraction. Yet even that represents an immense amount if judged by such quantities as we are in the habit of considering. At the distance of the earth, the amount above mentioned would correspond to about one horse power for every square

of five feet the side on the earth's surface, supposing the rays received perpendicularly, or in other words supposing the sun in the zenith; and in case the sun be not vertical, then to one horse power for every area of the size of the shadow of a square board of five feet each side held perpendicularly to the sun's rays. Imagine the earth's surface, whether land or water, studded over with horses as close as above mentioned, all working away with their full strength, and what an immense amount of working power we should thus obtain.

Figures such as these prepare us to expect effects on a grand scale as referable to light. Let us dwell on some of these. The supply of the warmth so essential to the life and growth of plants and animals (though for vegetation as we shall see warmth alone would not suffice) has already been mentioned. But as an accompaniment of this supply physical changes are produced in the atmosphere of the earth which are of vital importance. The surface of the earth, warmed by the sun's rays, in its turn warms the air in contact with it, and generally the lowest portions of the atmosphere. These are it is true to a certain extent warmed directly by the sun's rays, of which a portion, consisting mainly of rays of low refrangibility, are absorbed in their progress towards the earth. But the heat which radiates from the warmed earth

consists entirely of rays of low refrangibility, and a far larger proportion of it than of the direct radiation from the sun is affected by absorption in its passage; and thus it is that even independently of the heat communicated by contact from the earth to the air, it is chiefly the lowest portions of the atmosphere that thus get warmed. The warmed air becoming specifically lighter, the equilibrium is disturbed, the heated air ascends, giving place to colder air from above, which gets warmed in its turn. These ascending currents take place at first on a small scale, as we see over a boulder in a grass field on which the sun is shining; and the mixture thus continually going on has the effect of rendering a thicker stratum over the earth's surface warmer, and thereby enabling exchanges to take place on a larger scale. The effect of ascending currents on a larger scale is seen in the phenomenon of land and sea breezes. Where air thus ascends, a horizontal flow must of course take place below, to supply the place of the air which ascends. And when the distance that the air has thus travelled becomes large, another consideration enters which is of great importance in relation to the magnitude of the motion. In consequence of the earth's rotation, any point at the surface describes in the course of the 24 hours a circle of which the radius is the perpendicular let fall from that point on the earth's axis, and it has accord-

ingly a velocity from west to east varying from about 1000 miles an hour for a point at the equator to nothing at the poles. When air which moves along the surface to supply the place of other air which continuously ascends in some different region has travelled some distance in a north or south direction, it gets to a place where the velocity from west to east of the surface of the earth is very decidedly different from what it was at the place from whence the air came; and supposing the air to have been at rest relatively to the surface at the place from whence it started, and accordingly to have been moving with the velocity due to that latitude, so that to a spectator at that place there would be a calm, when it got into the different latitude it would tend by its inertia to preserve the velocity from east to west that belonged to its original latitude*, and accordingly relatively to a spectator at the new latitude to have a velocity from west to east or the reverse according to circumstances.

* This statement is not to be interpreted too strictly, as giving more than a general explanation. If an isolated particle were projected on the surface of a smooth gravitating sphere, which we may suppose revolving, though that would not affect the particle's motion, it will describe a great circle with uniform velocity, so that the component perpendicular to the meridian passing through it at any moment would vary as the secant of the latitude, until at the highest latitude attained it became equal to the whole velocity. The variations referred to in the text consequent upon a change of latitude would therefore tend to be even greater than if the velocity in a direction perpendicular to the meridian tended to remain constant.

There is on the whole an ascent of the air in the tropical regions, and the air which has ascended over-flows in the upper regions and proceeds towards the poles, while the place of the air which had so ascended is supplied by a flow in the lower regions from higher latitudes. In these movements on a large scale the change of latitude is considerable; and accordingly the difference of velocity from west to east belonging to different latitudes forms a very important factor in determining the actual velocity of the air near the surface at any place relatively to the surface, which constitutes the velocity of the wind that we observe.

It would be beside my object to attempt to enter into the question of the complicated system of currents in the atmosphere; I merely want to call attention to the circumstance that the whole system of winds in our atmosphere has its origin in solar radiation; has its origin accordingly in light. Without light, the atmosphere would be in a state of stagnation, and there would not be a breath of wind to fill the sails of our ships. Perhaps we might say, we should have recourse to steam. Nay, steam would fail us too, for a reason which will be explained in due course; nor failing steam could we have recourse to windmills, which would be useless in a perpetual calm.

When we stand on the coast of the Atlantic, and watch the huge waves which come rolling in and

dashing against the rocks in a gale, or it may be as the result of a gale out at sea which never reached us, we are struck by the grand exhibition of mechanical power which we behold. Yet all this power is but a minute fraction of the energy of light which started it; for the waves are due to the long continued action of the gale on the surface of the ocean, and the gale had its origin in currents of convection which themselves originate in solar radiation, that is, in light.

For fear I should be misunderstood in the assertion which I have just made, permit me to make a slight digression. I have said that of the sun's rays which are traversing the atmosphere on their way towards the earth a portion consisting mainly of rays of *low* refrangibility are absorbed in their progress. It may have occurred to some of you to ask whether I should not have said rays of *high* refrangibility. For when the sun is near the horizon and his rays have accordingly a comparatively long space to travel through the atmosphere before they can reach us, the light we receive is orange or red, as we know familiarly from watching sunsets; and examination by the prism confirms what is already shown by the colour, that it is the rays of *higher* refrangibility chiefly that are missing.

In explanation of this apparent contradiction, it must be observed that there are two perfectly distinct

causes why the light should fail to reach us:—one, absorption in the strict sense of the word, where light as such is swallowed up, and in lieu of it we have a warming of the air where it is so absorbed; the other a mere scattering of the light by very fine particles of water forming a mist, or by particles of whatever kind they may be which are similarly held in suspension.

When the suspended particles are excessively fine they offer proportionately more obstruction to rays of small wave length and accordingly high refrangibility than to rays of greater wave length and accordingly lower refrangibility. But in this case the obstructed light is reflected back into space (the reflection being of course complicated by diffraction); and light cannot warm a body without disappearing as light and being absorbed. Hence though the obstruction of light by solid particles may greatly exceed its obstruction by true absorption, it does not at all follow that the warming produced by the total obstruction is divided between different parts of the spectrum in anything like the same proportion as the total obstruction. Of course the suspended particles may themselves absorb light, as in a London fog, but I had not in view such purely exceptional cases as that of the smoke about large towns.

After this digression let me resume my subject.

Concurrently with the warming of the earth's surface, another process is going on which is fraught with consequences of the highest importance. By far the greater part of the earth's surface is covered with water, and the surface of the land is in most places more or less moist. Consequently under the influence of the solar radiation evaporation is constantly going on; water is constantly converted from the condition of a liquid into that of an elastic fluid, which mixes with the air. The quantity of water which can be thus sustained in the elastic state in a given volume depends upon the temperature; and when the mixture of air and vapour is sufficiently cooled, whether by radiation, or by the cold accompanying expansion as it ascends, or by being brought in contact with the surface of the earth where cold, or being mixed with colder air from some other quarter, a portion of the water is condensed, forming a vast number of exceedingly minute globules, which fall so slowly in consequence of the resistance of the air due to its viscosity that they may be deemed for all practical purposes to be permanently suspended, and which form the clouds; and when the precipitation is sufficient, or under other circumstances not wholly understood, these globules unite to form larger spheres, and fall as rain. This supply of water, so essential to the growth of plants and the sustentation

of animals, is as we see another of the all-important effects brought about through the agency of light. The quantity of water thus raised and let fall again, taken all the earth over, is prodigious. In a dry season we may water the flowers in a garden, but we do not think of watering the fields. Even supposing we had a supply of water at hand, the labour involved in raising, carrying and distributing that water would be so great if we would make any sensible impression that it would not pay, and the fields are left to their fate. An inch of rain, which occasionally may fall in a single day, means 100 tons per acre, and think of the labour of raising and distributing such a weight as this. Yet all this is done for us through the energy supplied by light. And when the fallen rain has fertilised our fields, the overplus collects in rivulets and streams which afford a constant supply of water to men and animals, are used occasionally for supplying energy in water mills, and ultimately collect in rivers which afford one of the cheapest modes of transit for merchandise. When we stand by some mighty waterfall, such for example as Niagara, and are struck by the grand exhibition of power that we see before us, we do not perhaps reflect that while it is through light that we are enabled to see what is going on, it is from light also that the energy is derived that we see thus in action.

The chief effect of light that I have hitherto in this lecture dwelt upon is that of raising the temperature of the body on which it falls. Among the effects which at the beginning of the lecture I enumerated as capable of being produced by it, there is one, the production of phosphorescence, which at first sight might seem to be merely a matter of scientific curiosity, having little relation to our well-being. And yet there is one effect constantly going on, and closely connected with our welfare, which if not identical with is at any rate very closely allied to phosphorescence.

For what is phosphorescence? The term it is true is applied to more phenomena than one, and that from which the name is derived, the shining of phosphorus in the dark, has nothing to do with that to which the name is more commonly applied, and which has been mentioned as an effect of light. This latter consists in the shining of certain substances in the dark, or even in many cases in fairly strong light, after exposure to light of suitable kind and intensity. The duration of the effect after the incident light has been cut off varies immensely in different cases. Sometimes the body will go on shining for hours; sometimes the effect ceases to be sensible at the end of a few seconds; sometimes it remains for only so small a fraction of a second that it requires special

instrumental appliances to prove that there is any duration at all; sometimes the duration is shorter even than this, so that it has not at present been rendered sensible in experiment, and it is only from analogy and as a deduction from what we have reason to believe to be the theory of the effect, that we infer that there is any duration at all.

As regards the relation of the light given out to the light that falls upon the body and is the cause of its emitting light, this law appears to be general— that the light given out is of lower refrangibility than the light which excites it It is true that sometimes a solar phosphorus emits light in consequence of being shone upon by light of lower refrangibility than that which it emits; but this effect is only temporary, and depends upon a latent condition in which the body was left in consequence of the previous action of light of higher refrangibility than that given out; and when this effect of the higher rays is exhausted, the body refuses to shine though left under the

* When rays of low refrangibility are enormously concentrated they are capable of heating a body so as to make it red-hot, and accordingly cause it to give out rays of higher refrangibility than themselves. This effect, the "calorescence" of Dr Tyndall, is not here under consideration. It is doubtful at present whether the law enunciated in the text is *rigorously* true; some physicists maintaining that in certain cases of "fluorescence" light is emitted of refrangibility extending to *a little* above that of the incident rays, while others hold that that is not the case. Such slight deviations from the law, even if real, will not affect the conclusions drawn in the text.

influence of the lower rays which at first excited it. The exception therefore to the rule first laid down is more apparent than real.

Now when we leave out of sight the distinction between the visible and invisible rays, a distinction which is purely subjective, depending on our own organization, not on the nature of the external object, that is, the radiation presented to us, we find an exceedingly common phenomenon presenting, in some respects at least, a close resemblance to phosphorescence. I refer to the emission of radiant heat as a result of a body's having been shone upon by the sun.

When a body, a stone for instance, is warmed by absorption of the sun's rays, it radiates in its turn, and becomes a source of what in the extended sense of the word may be denominated light. But this light does not affect the eye; it is of lower refrangibility than the extreme red, and its capacity for passing through or on the other hand being absorbed by gases or liquids or homogeneous solids is altogether different from that of the rays to which it owed its origin. Now in the solar radiation, at any rate as it reaches us, a far smaller percentage is made up of invisible ultra-red rays, of refrangibilities which even for such rays are low, than is the case with most terrestrial sources, even flames; while on the other

hand the radiation from a body of moderate temperature, such as a warmed stone, consists almost entirely of rays of these very low refrangibilities. But air transmits very freely the visible rays, and also invisible ultra-red rays of not too low refrangibility, and therefore the bulk of the solar radiation passes very freely through it. On the other hand air, in its ordinary state, has a considerable absorbing power for the ultra-red rays of comparatively low refrangibility, which are accordingly stopped, and the temperature of the air is raised thereby. I say air "in its ordinary state," for Tyndall has shown that dry air is highly transparent, even for rays of these low refrangibilities, and that among the normal constituents of the atmosphere it is mainly vapour of water to which the observed absorption is owing; but this is always present more or less, though the quantity contained in a given volume of air may vary very greatly according to circumstances.

Accordingly while the solar radiation falls freely on the earth (assuming of course that there are not clouds in the way to stop it), the radiation from the bodies which it warms is in far greater degree arrested, and goes to warm the air. The atmosphere thus to a certain extent plays the part of the glass in a greenhouse, and the heat is husbanded, to the advantage of those at least who live at a good

distance from the equator, not to mention the all-important results of the heating of the atmosphere which have already been dwelt upon. And if we may indulge in speculation as to the condition of a distant body, the telescopic appearance of Jupiter, and his high "albedo," or intrinsic whiteness, favour the idea that his body is mostly enveloped in a dense mantle of cloud, which is absent or much reduced only along the comparatively narrow "belts," where, it may be, we see down to the body of the planet, or at any rate to a considerable depth in a furrow of the ocean of cloud. Being in round numbers four times as far from the sun as we are, Jupiter would receive a radiation only the one-sixteenth part as intense; but it may be that the mantle of cloud acts as an effectual great coat, husbanding the heat derived from such part of the solar radiation as is able to get through it, or at any rate some way into it, perhaps also husbanding some remains of primitive heat. But this as I said is matter of speculation, and I forbear to enlarge on it, being desirous of resting what I have to say to you on well-ascertained facts.

The nature of the subject in which we have been engaged to-day afforded me but little of novelty to bring before you, and I cannot help fearing that I may have been somewhat wearisome in consequence. Still I am not without hopes that I may have led

some of you to appreciate more fully than before the inestimable benefits we owe to light in rendering our earth suitable for a habitation of living beings, even supposing they could themselves exist independently of light. In my next lecture I propose to show how essential light is even to their very existence.

LECTURE II.

Dependence of the various sources of warmth which are available to us on Light—Dependence of the energy of animals on their food, and of the potential energy of the food on the energy of Light—Decomposition of carbonic acid by growing plants—Rays which effect the decomposition—Changes of colour of leaves under the influence of Light.—Effect of an extra supply of Light—Condition of the earth without Light.

ONE of the effects of light which I mentioned in my first lecture is that of producing chemical changes. When the benefits derived from light which depend on the chemical changes which take place under its influence are mentioned, perhaps our thoughts turn to photography, an art which has sprung up within the memory of many now living. How marvellous would it have been thought a century ago that we should be able to obtain in the course of a few seconds, by a purely automatic process, an absolutely authentic picture of a striking landscape of which we

wish to preserve a memorial, or portrait of a friend or family group. It is true that hitherto these pictures have been shown only in light and shade. Some little progress has indeed been made towards photographing with colour. M. Edmond Becquerel has succeeded in obtaining on à daguereotype, by a particular process, a delineation of a spectrum the different parts of which showed colours having some resemblance to the colours naturally belonging to these same parts in the actual spectrum delineated. But hitherto it has been found impossible to fix the colours so obtained. Occasionally too indications of colour, chiefly I believe of red, have been obtained in ordinary photographs, but accidentally as it were, doubtless by some variations in the chemical processes involved, or in the mode of exposure, the conditions of which have not been investigated. Enough has been done to show that the idea of photographing with colour may not be wholly chimerical.

But interesting as photographs may be, and glad as we may be to possess a perfectly faithful likeness of a friend or relative, perhaps far separated from us by distance, perhaps removed by death, the advantages we thus derive sink into insignificance compared with others the reference of which to the chemical agency of light is not so obvious.

In my first lecture I pointed out how the source of at least by far the greater part of the warmth which we enjoy is derived from solar radiation. Supposing we were bereft of this, what sources of heat would be available to us?

Perhaps we might say, at least we should have the natural animal warmth. Much as we might suffer from cold, perhaps by warm clothing, and by keeping near one another, we might manage to get on.

But clothing does not originate heat; it merely checks the waste of that which is in some way supplied through the animal organisation. What then is the source of this supply? It is well recognised at the present day that it is derived from the chemical changes which take place in the food. After digestion in the stomach and alimentary canal, the portions available for nutrition are carried into the blood, and circulate through the body. A portion goes to supply the waste of the tissues, but a large part acts as fuel, and by gradual union with oxygen, in fact by slow combustion, furnishes a gentle and continuous supply of heat. At every breath, a portion of the oxygen inspired is absorbed by the blood, which gets access to it in the cells of the lungs. This oxygen enters in the first instance into some sort of chemical combination with the colouring

matter of the blood, which now becomes a powerful oxidising agent, and in the course of the circulation oxidises matters contained in the blood. As regards its ultimate elements, the food consists mainly of carbon, hydrogen, oxygen, and some nitrogen; but the quantity of oxygen is not near sufficient to form carbonic acid and water with the carbon and hydrogen respectively which are present; and as the two latter elements are eliminated mostly in these two forms, we infer that the final result is that the absorbed oxygen unites with carbon and hydrogen to form carbonic acid or water as the case may be. In this process heat is given out, just as in actual combustion; so that a sort of slow combustion is always taking place in the lungs and in the circulating blood, resembling actual combustion as regards the heat given out, but differing from it as to the rate at which the chemical changes take place, and not like it accompanied by luminosity and a high temperature.

In carbon and oxygen separately presented, or in hydrogen and oxygen, we have a supply of energy in the potential form; energy which may become actual, and appear it may be in the form of heat, it may be in the form of work done, when the potential energy becomes actual by chemical union, whether quick or slow. Conversely, carbonic acid cannot be converted

into carbon and oxygen, nor water into hydrogen and oxygen, without the expenditure of energy in some shape or other.

In seeking for a supply of warmth in default of solar radiation, we have been driven from animal heat to the food of animals, food which contains carbon and hydrogen in great excess of the quantities which by union with the oxygen which the food also contains could form carbonic acid and water. Now the food of animals is derived directly or indirectly from vegetables, indirectly in so far as it consists of the flesh of other animals, through which we come down to vegetable feeders at last.

From whence then do plants derive their carbon and hydrogen? As to hydrogen there is no difficulty. Though it is not found free, and though there is no reason to suppose that plants could assimilate it if it were, it abounds in nature as a constituent of water, which the plants readily imbibe. As to carbon, it exists to a certain extent in the surface soil in which plants usually grow; but it is there in an insoluble state, a state in which the plants could not assimilate it. It is now well ascertained that the source of the carbon found in plants is the carbonic acid which is contained, though in small proportion, in the air.

But the mere absorption of carbonic acid and

water would not suffice. They are the ashes, so to speak, which result from the combustion of carbon and hydrogen, and would not be competent in association with oxygen to become a source of energy. How is it then that in plants these elements, in opposition to their chemical affinities, are presented to us in combination with a much smaller proportion of oxygen than in the state in which they were absorbed by the plants?

We are here brought face to face with a very marvellous process, a process which is only imperfectly understood. It is found that living plants when supplied with moderate quantities of carbonic acid, in addition to the water which they have in them or which is supplied from without, are actually able to set oxygen free, and appropriate the carbon and hydrogen, not indeed as such, but in a state or states of combination in which they are combined with much less oxygen than they originally had. But this process *only goes on under the influence of light.* It is, as has been noticed, the very reverse of combustion. In combustion whether rapid or slow, energy is given out; in the reverse process energy must be supplied. It appears from what has been mentioned that this energy is derived from light. To prove this indeed by direct measurement would be a matter of extreme delicacy and difficulty, and I am not aware that the

experiment has been attempted. But even without such direct verification the proposition may be regarded as thoroughly well established from what we know at the present day.

This process may be watched by means of a simple experiment which any one can make. Let a few fresh green leaves be put into a vessel of clear glass, such as a finger-glass, or a wide-mouthed bottle, which is filled with water, and inverted in a vessel of water, the water used containing a little carbonic acid in solution, either naturally present or purposely introduced. If the whole be now exposed to the rays of the sun, it will be found that bubbles of gas are formed at the surfaces of the leaves, and these bubbles grow larger, become detached, and rise to the top of the vessel. If now the collected gas be for convenience poured under water into an inverted bottle with a narrow neck filled with water, and this be now stopped with the finger, taken out, held upright, and an extinguished but still glowing splinter of wood be introduced immediately on removing the finger, the red end of the splinter bursts into vivid combustion, indicating that the gas was oxygen, or at least contained oxygen in much larger proportion than in the air.

We see thus how the energy of light becomes in part stored up in plants, which in this way become

fitted to be food for animals, man included, by whose muscular movements energy is again given out. For long geological ages this must have been the form in which alone energy was given out by animals. But now that man has appeared upon the scene, and civilization has advanced and science made progress, such stores of energy which have lain buried for ages are opened up to him, and daily employed by him in the execution of the works which he carries on. Far back in geological time the earth teemed with a luxuriant vegetation, of forms which have mostly passed away. The remains of this vegetation, accumulating on the spot where the plants grew, subsequently became buried in the course of the changes which the earth's crust has undergone, and being modified by pressure, and the chemical changes which decaying vegetable matter undergoes, they now form those coal-fields to which our own country owes so much of its greatness, and which we daily make use of in all sorts of ways. The working power which draws our railway trains, and enables us to travel at a rate which our ancestors would have thought chimerical, that which propels our steamers, and carries our merchandize and our travellers across the ocean in spite of calms or adverse winds, that by which the massive engines with which these steamers are furnished were fabricated, the heat and reducing carbon by which the iron of

which those engines are made was obtained from the ore, the fires with which we warm our apartments, the gas with which we light our streets, nay even the working power that drives the dynamo machine that supplies us with electric light—all these are derived from the energy of light, stored up in remote geological ages, and now placed ready to our hands.

I have spoken of the fundamental process of the appropriation of carbon from the air by growing plants under the influence of light. But in order that it should be appropriated it must previously have been in the atmosphere, where it is found as we know in the shape of carbonic acid. The supply of carbonic acid contained in the air is constantly drawn upon by growing plants. A small portion is restored at night, for in the dark plants give out a little carbonic acid, and some more is given out in the chemical changes which accompany decay. But the supply thereof from this source is far from meeting the demand, so that if there were no other the carbonic acid in the atmosphere would be exhausted, vegetation would cease, and animals would die of starvation. But in the respiration of animals the opposite process is continually going on, carbonic acid is given out in the lungs, the carbon of which is derived from the vegetables which directly or indirectly form the food of the animals, and fires contribute to the same result. Thus by the

coexistence of plants and animals the requisite balance is kept up. Without animals the supply of carbonic acid which is essential to vegetation might in time be exhausted: without plants, supposing that animals could otherwise obtain their food, the atmosphere would in time become so charged with carbonic acid as to be unfit for respiration.

What the particular chemical process may be by which oxygen is removed from carbonic acid by plants under the influence of light, is not at present known. It is believed to be in some way intimately connected with chlorophyll, as the green colouring matter of leaves is called. This substance is known to be a mixture; thus the chlorophyll of land plants consists mainly of three colouring matters; one bluish green, called by Sorby blue chlorophyll, which yields solutions exercising a powerful and characteristic absorption, and exhibiting a lively red fluorescence, another, probably greenish yellow, called by Sorby yellow chlorophyll, possessing very similar characters, though easily distinguishable by its mode of absorption, and another called xanthophyll which yields solutions which exercise a very distinctive mode of absorption in the blue, and are not fluorescent. It is remarkable that if we except the fungi, which we may regard as vegetables of prey, and perhaps a parasite or two, this mixture is always present; though not in all

parts of a plant, nor even always throughout the leaves. Thus some variegated leaves are white in parts, and these parts are free from chlorophyll, or yellow in parts, in which parts the green bodies may be absent or nearly so. On the other hand leaves may differ in colour altogether from green, not from the absence of chlorophyll, but from the presence of some other colouring matter which masks its colour. Thus red cabbage owes its red colour to the presence of a red colouring matter, which differs altogether in nature from chlorophyll, being soluble in water; and when this is removed it is found that red cabbage contains chlorophyll just like green leaves in general; and even when we descend to the seaweeds we still find chlorophyll, not it is true in the olive and red series identical with that of land plants, except as regards one constituent of the mixture, namely blue chlorophyll, but this is the most important constituent as an element of coloration.

This universal presence of chlorophyll in self-sustaining plants indicates that it fulfils some very important office, and leads us to suppose that it is connected with that all-important function, the appropriation of carbon and elimination of oxygen.

The absolute necessity of light for the life of self-sustaining vegetables may be illustrated by an easy experiment. Let seeds, say of cabbage or turnip, be sown

in pots which are placed in a dark cellar. Presently the seeds germinate, and send up stalks surmounted by a small pair of seedling-leaves, which however are yellow instead of green. If now the pots be left where they were, the stalks grow longer and longer, remaining however thin, white and watery, and the little pair of seedling leaves at their summits seem to grow no more, and retain their yellow colour; and presently the stalks can no longer sustain their own weight, and fall over, and the little plants die.

But if when the seeds have germinated a pot be carried into the open air, so as to be freely exposed to the light, and left there, the result is very different. The yellow seedling leaves soon become green, and the growth of the plant continues in the usual manner.

The effect of an extra supply of light on vegetation was strikingly shown by an experiment, the result of which was exhibited by the late Sir William Siemens at a meeting of the Royal Society of London. Three pots were sown at the same time with seeds of the same kind, mustard I believe. One was kept under the usual conditions, having daylight by day and being in the dark at night. Another was kept in darkness during the day and exposed to the electric light at night. The third had daylight during the day and electric light all night. The plants in the first two pots were much alike, but

those in the third pot which had light night and day were strikingly different. The plants were stouter looking, with larger leaves, and of a darker green, but they were not so tall.

This experiment illustrates a phenomenon with which those who have sojourned in the Arctic regions (not too far north) have been struck, namely, the rapidity with which vegetation comes on when once summer has fairly set in. In those high latitudes the sun of course remains above the horizon in summer-time for by far the greater part of the 24 hours; and so the growing plants are somewhat in the same condition as the plants that were worked day and night in Siemens's experiment.

These two processes, the elimination of oxygen and appropriation of carbon, and the turning green of the yellow leaves of etiolated plants, take place as we have seen only under the influence of light. But light is heterogeneous, and in the wide sense in which the word has been defined contains not only visible rays differing from one another in colour and refrangibility, but also invisible rays lying beyond both ends of the visible spectrum. An interesting question now arises, are all these rays efficient alike in the production of these two effects, or of either of them, and if not, in what manner is the activity in either case distributed in the spectrum?

This question has been subjected to experiment by several persons, and notably by the late Dr John Draper, and at his suggestion by Dr Gardner, who worked with all the advantage of a Virginian sun. Instead of trusting to coloured glasses, the light transmitted by which is of a compound character, and usually includes invisible rays from one or both ends of the spectrum, they used sunlight which was reflected horizontally into a perfectly darkened room, and passed through a prism placed in the window. The spectrum thus formed was not it is true pure, but still the colours were not greatly mixed, and it could only be quite neighbouring parts of the spectrum that overlapped. The result was very decided; the evolution of oxygen by green leaves placed in water charged with carbonic acid, and the turning green of leaves which had germinated in the dark, and so before exposure presented only yellow seedling leaves, both one and the other were strongest about the brightest part of the spectrum, about the greenish yellow, and from thence decreased in both directions; the blue and violet rays in particular, which act so powerfully on most photographic preparations, being almost wholly if not wholly inactive in producing the phenomena now under consideration. The accordance between the parts of the spectrum which produce these two effects respectively makes it probable that the

process by which oxygen is separated from carbonic acid under the influence of light has for result, or at least for one result, the formation of the green colouring matter.

Besides the greening of the yellow leaves of etiolated plants there is another action of light in relation to chlorophyll which we shall do well to consider, as having in all probability a most important connection with the growth of the plant. The mixture of substances called chlorophyll is soluble in alcohol, and if the mixed solution be exposed to light it is soon bleached, and there is nothing left whereby chlorophyll or products of its decomposition can be traced by their peculiar action on the spectrum. In order to insure the success of this experiment, it is necessary to be careful to guard against any decomposition of the chlorophyll, especially by even a trace of acid. For acids decompose it extremely easily, and the products of decomposition, at least of the green bodies, show like the parent substances a powerful and highly distinctive absorption (though differing from that of the parents) and a red fluorescence, but unlike them are bodies of great stability, and in particular are not readily affected by light. The same action of light in discharging the green colour is found in the chlorophyll grains themselves; and the action of the different parts of the spectrum in producing this effect

was studied by Sir John Herschel, who received a spectrum on paper coloured green by crushed leaves. He found that the effect was mostly confined to the visible spectrum, or at least parts of it, and that the most powerful action of all took place far on in the red, at the place where chlorophyll exercised its most energetic absorption.

Now is there any natural phenomenon attending the growth of plants which puts in evidence this power of light to discharge the green colour? I believe there is. We are familiar with the change from green to yellow which takes place in the leaves of trees shortly before they fall in autumn. We are not to suppose, as we might at first sight have been disposed to do, that the green substance is changed into a yellow. Chlorophyll is, as I have said, a mixture, the constituents of which can be more or less completely separated from one another by suitable means. In this way it can be shown that the yellow substance found in the sere leaf was there already; the change consisted in the disappearance of the green constituent or rather constituents of the mixture—those constituents which exhibit the powerful absorption in the red and the red fluorescence. Now as it is only by light that we are able to discharge the colour of the green constituent without the formation of products of decomposition which the prism at once detects, unless

indeed we have recourse to somewhat violent chemical agents, the like to which we can hardly suppose to be naturally present, we are authorised to conclude, at least with the highest probability, that the disappearance of the green constituent in the autumnal change is due to the action of light.

But if these changes from yellow to green and from green to yellow so readily take place, why is it that during nearly the whole of the life of a leaf it retains its green colour unaltered? The most natural supposition to make is that the two processes are always going on simultaneously; that there is a perpetual formation and a perpetual destruction of the green matter under the influence of light. This mode of viewing the change leads us to suppose that the green which is constantly present is not identically the same matter throughout, but is rather a phase or state of chemical combination through which matter passes, different molecules of matter in succession, probably on the road from the inorganic forms of water, carbonic acid, nitrates &c., to the organic chemical constituents of which the plant is made up.

But be the steps of the process whereby plants are enabled to assimilate carbon and eliminate oxygen what they may, this much is certain, that it is only under the influence of light that they are able to do so. Let us pause now to consider some further con-

sequences, besides those dwelt on in the first lecture, which would follow from the absence of light. It was then pointed out how we should be deprived of our main source of warmth. But even subsidiary sources would fail us too. Animal heat would not be available, for animal heat and life are dependent upon food, and food we could not have without plants, and plants could not grow without light. Fires we could not have, for the wood or peat or coal which we might burn is derived from plants. The atmosphere would be in a state of stagnation; no wind to impel our ships, if ships we could have; nor in the absence of wind would steamers be available, for our steamers are impelled by means of coal, and what is coal but the relics of extinct vegetation, dependent therefore for the energy which in association with the oxygen of the air it supplies on the energy derived from sunshine, we know not how many thousands of years ago? In the absence of sunshine, of fuel, of wind, of rain or descending streams, we might perhaps think of tide-mills as a conceivable source of energy. But this too would fail, for the whole earth would be in a worse plight than the Arctic regions, and the ocean would be covered by more than palaeocrystic ice; would in fact be a frozen mass. Our earth would be a silent abode of darkness and of death, the stillness only interrupted by the occasional noise produced by vol-

canic explosions, and the darkness by the occasional lurid glare in some few places of lava streams issuing from a volcano, or of the red hot stones which it ejects; though even this there would not be if not only were our earth destitute of sunlight, but there were no such thing as light at all.

LECTURE III.

Light as subservient to vision—General structure of the eye in relation to the formation of images—Theoretical imperfections of the image formed of no practical importance in a normal eye—Structure of the retina—Rods and cones—Probable seat of the perception—Photochemical changes in the retina—Analogy of fluorescence—Power of adjustment for focus—Perception of colour—Theory of distinct primary sensations of colour—Coloured globules: their possible function—Single vision with the two eyes—Theory of corresponding points—Muscles for turning the eye-ball in all directions—Functions of these muscles in procuring singleness of vision.

WE so habitually use light for our guidance, for informing ourselves of what is passing at a distance, for enriching our minds with the thoughts of others by reading, for conveying information by writing to those at a distance, that we probably think of such things as these as forming the use of light. Yet inestimable as these uses are, there are others of still

more vital importance; and in the two preceding lectures I have endeavoured to point out how absolutely essential light is to our very being, constituted as we are; how without it the earth would be a silent abode of darkness and of death.

But supposing light to be supplied as it is, and that under its influence plants grow and animals are fed on them, and obtain the warmth which they require, still light would be useless for purposes of guidance and information were we not furnished with organs adapted to its reception and to the utilisation of it for such purposes as those above mentioned. To-day I would draw your attention to some points connected with the construction and functions of that marvellous organ for the reception of light with which we are furnished. I shall mainly confine myself to the human eye.

The general construction of the eye is so familiarly known that I need not dwell upon it. The eye-ball is approximately spherical, fitting into a bony socket lined with fat and connective tissue, in which it is free to turn in all directions with hardly any friction. It is invested with a very tough covering, the sclerotic. The investing membrane is mostly white and opaque, but in front it is beautifully transparent, forming the cornea. This front portion is not exactly a continuation of the general nearly spherical surface of the eye-

ball, but is slightly more protuberant, so that its surface resembles that of a prolate spheroid of revolution rather than that of a sphere, the axis of revolution being the axis of the eye. Unlike the other parts of the body, which are opaque, or merely translucent, the body of the eye is beautifully transparent and colourless. It is divided into two chambers by the crystalline lens and its support; the anterior chamber being filled with the so-called aqueous humour, the posterior with the vitreous humour. The refractive indices of these humours very little exceed that of water. The crystalline lens, which is shaped in a general way very much like a lens formed by the optician, is more refractive, its refracting power placing it at about two-thirds of the way from water to crown glass. It has been found that the refractive power of the substance of which the lens consists, varies a good deal in different parts, increasing on the whole from the external layers to the centre. The breadth of the pencil of light from any luminous point which enters the cornea, in proportion to the diameter of the eyeball, is enormous compared with anything we have in a telescope, but it is stopped down to a suitable aperture by an opaque screen, the iris, in the centre of which is a hole, the pupil, circular in man, though of different forms in some other animals, for example cats and horses. It is hardly necessary to mention

what is so familiarly known, that the pupil contracts or expands spontaneously, that is involuntarily, according as more or less light enters the eye, remaining, in man, circular at all sizes.

Up to the formation of distinct images on the retina, which, as is well known, is the condition of distinct vision, the eye acts simply as an ordinary optical instrument, and we can give a full account of its functions according to the ordinary laws of refraction. In a similar optical system formed of media bounded by spherical surfaces, the image of a point would be approximately a point, but would be a little diffuse, from the effects of spherical and chromatic aberration. The question arises, do these exist in the eye, and do they impair sharpness of vision?

First, as to spherical aberration. The principal refraction takes place at the surface of the cornea, where the light passes out of air into a medium slightly more refractive than water, whereas the media it encounters in its subsequent passage do not any of them greatly surpass water in refracting power, and therefore the refraction in passing out of one into another is not nearly so great, for equal angles of incidence, as in the former case. Now when light from a distant radiant point falls on a homogeneous medium bounded by a surface of revolution in the axis of which the radiant point is situated, it may

be shown that the form of the surface necessary to cause the refracted pencil to converge accurately to a point is that of a prolate spheroid of revolution, the axis of revolution passing through the radiant point, and the eccentricity of the generating ellipse being the reciprocal of the refractive index. Now it is remarkable that the cornea is approximately just such a surface.

Again, the defect of spherical aberration in an ordinary lens bounded by spherical surfaces, when used for refracting to a real focus light proceeding from a radiant point in its axis, consists in this, that the rays which fall nearer to the edge are too much refracted to be brought exactly to the same focus as those refracted nearer to the centre. One way in which this defect might conceivably be remedied would be by making the density of the medium increase, in a suitably continuous manner, in passing from the edge to the centre. It is not within the power of the glass manufacturer and of the optician to produce such a lens, but in the crystalline lens of the eye we have one of that character, and accordingly more or less approximately fulfilling the desired condition.

The result is that, though there is still a certain quantity of residual spherical aberration, its amount is not such as to cause any serious departure from

perfect sharpness of definition in the image; in fact, it is only by somewhat refined modes of observation that we can establish the existence of that residual spherical aberration of which I spoke.

Next, as to chromatic aberration. This arises from the fact that when light enters glass, water, &c., the refraction increases from the red to the blue, and therefore in a convex lens the blue rays are brought to a focus before the red, and the rays of intermediate refrangibilities at intermediate distances, so that at no one distance are all the rays collected in a common focus. Hence at the best focus for the rays coming from a radiant point of white light, that namely where the rays from the brightest part of the spectrum are collected, the red and the blue rays are somewhat diffused, the red rays not yet having reached their focus, while the blue rays have passed theirs. Before the discovery of the different dispersive power of different media, this constituted a most formidable obstacle to the improvement of telescopes. Object glasses of most inconveniently great focal lengths were used, in refracting telescopes, in order to diminish the imperfections of the image which were due to chromatic dispersion. For example, an old object glass of Huyghens's is in possession of the Royal Society of London which is 9 inches in aperture and no less than 122 feet in focal length. Nowadays a telescope

of similar aperture would be made with a focal length of about 9 feet only. If then chromatic dispersion caused such a serious inconvenience in the construction of telescopes, and the eye we know acts as a telescope, is there some such compensation in the eye as that which we have in the achromatic object glass, and if not, must not the want of it be a most serious inconvenience?

To the first question the answer is, there is no such compensation in the eye, and the eye is not achromatic. A pretty way of showing this is by throwing a pure spectrum on a page of small print, in an otherwise darkened room. At the ordinary distance of reading the words are seen quite sharply in the brightest part of the spectrum, but somewhat indistinctly in the red from long-sightedness, and very indistinctly in the violet from short-sightedness.

Are we then to regard the want of achromatism as an imperfection in the eye? The answer to that question depends on what we regard as the object of the eye. If the answer be, to guide us in the daily wants of ordinary life, then the want of achromatism ought not to be deemed an imperfection unless it diminished the sharpness of vision for ordinary purposes. But in reality it requires some rather refined experiments, such as that mentioned above, or some very unusual condition as regards the use of

the eyes, such as viewing objects through a deep cobalt blue glass, to render the fact of the want of achromatism in the eye sensible at all. Of the twelve hundred millions or so of human beings who inhabit our earth, how few are the philosophers who concern themselves with optical experiments! And for those who do, what a small portion of their time is occupied in experiments of the kind compared with the time during which they are using their eyes for common purposes! And even in experiments there are very few indeed where the non-achromatism of the eye is any disadvantage; and if in some special case inconvenience should arise in the first instance, the man has only to set his wits to work to devise some plan for getting over it. As regards the ordinary requirements of life, the want of achromatism in the eye is of no consequence whatsoever.

Up to the formation of images on the retina we can fully explain the functions of the eye, provided at least, so far as the iris is concerned, that we confine our attention to the effect produced by the varying limitation of the pencil, and do not mean to include an explanation of the mode in which the motions of the iris are brought about. But now we come to a part of the structure respecting the functions of which we have only a very imperfect knowledge; that part, namely, which is destined to receive the

impression of the external agent, and convey the influence to the sensorium.

The coating of the eye from the outside at the back of the eye-ball to the vitreous humour, is distinguished into three coats; the hard, white outer coat called the sclerotic, already mentioned, an intermediate coat called the choroid, and inside this again, between it and the vitreous humour, the retina. These coats are further subdivided into layers, the retina more especially presenting a highly complex and curious structure. I am no anatomist or histologist myself, and I do not propose to describe to you at second hand, except very briefly, the microscopic structure of this curious organ. As we proceed from the centre of the eye-ball outwards, that is, towards the socket, and accordingly in the direction in which the light travels, we first meet with a plexus of extremely fine nerve fibres, the general course of which is parallel to the surface of the eye-ball, and which unite in the optic nerve, which runs into the brain. This layer of nerve fibres is followed by several other layers, containing "granules," or "ganglionic cells," or "molecules," until at last we come to a remarkable and very peculiar structure, the "bacillary layer." This series consists of a set of elongated bodies, arranged radially, and closely set in lateral directions. They are of two kinds, which are de-

nominated respectively rods, and cones. Each of these consists of two portions, an outer, highly refracting portion or outer limb, and an inner portion or inner limb not thus highly refracting, the two portions further differing as regards the effect produced on them by certain reagents. Each inner limb on its inner side runs into a fibre, apparently a nerve fibre, which proceeds in a direction towards the nerve plexus, but runs into certain roundish bodies "granules" &c., on the way. On the other hand the nerve fibres of the plexus turn on the outer side into an oblique direction, running on in the direction of the rods and cones. There seems every reason to believe that the nerve fibres of the plexus are continuous with the fibres seen running out of the inner limbs, though the continuity has not actually been traced quite the whole way.

Consider for the present the action of a single eye, reserving for future consideration the joint action of the two eyes. We know that the image of a point on the retina gives rise to the sensation of a point in the field of view, and that the part of the field that the point seen appears to occupy depends on the part of the retina on which the image fell. The images of a variety of points in the field of view necessarily follow the same order of sequence with regard to lateral direction as that of the actual points from whence the

pencils come, the image of a point to the right of the axis of the eye falling on a point of the retina to the left of that on which falls the image of a point in the axis; the image of a point above the axis falling on a point of the retina below the axis, and so on. The order of sequence of the sensations is that of the sequence of the points of the retina affected; and that an inverted image should give rise to the sensation of an erect object need not create any difficulty, as there is no reason *a priori* that we can see why the order of the sensation as to up and down, right and left, should be the same as the order of the points of the retina affected rather than the opposite. It remains to enquire whether there is anything in the structure of the retina which appears calculated to give rise to separate sensations when separate points of the retina are stimulated, even though those points should lie very close to each other, and again whether there is anything which appears calculated to receive impression from light, at the expense of energy on the part of the light so impressing it, and thus to become an organ of perception.

Now there is just this one part of the retina, the bacillary layer, where we have a vast number of separate elements closely set in lateral directions, while elongated comparatively speaking in a radial direction. These elements are provided with fibres,

which, there is every reason to believe, are nerve fibres, running towards the layer of nerve fibres which forms the first layer of the retina as we proceed from the centre of the eye outwards. Moreover the lateral distance between the cones in the part of the human retina immediately about the axis, where the vision is most distinct, agrees very closely with the distance on the retina of the images of two points which can just be seen as two. It is found that the smallest angle subtended at the eye by two points which can just be seen as two is about 1′, from whence the distance of the two images on the retina can be calculated. It comes out about 4 thousandths of a millimetre, and the lateral distance apart of the cones near the axis of the eye is according to Schultze about 3 thousandths of a millimetre, or very little over the thousandth of an inch. There are still further arguments leading to the same conclusion, namely, that the rods and cones are the percipient organs.

I have said rods *and* cones; for that both are concerned in vision, which is possible with either, is shown by the fact that some animals (as for example lizards) have got only cones, while in others (as the owl) there is little else than rods, and in bats, nothing but rods. In the human eye, we have over the greater part of the yellow spot cones only, while

over the greater part of the rest of the retina the rods are much more numerous than the cones; and we know that while we see most distinctly over a small portion of the field of view surrounding the point to which we direct our eyes, we do see simultaneously over a very large field. But while it is certain that either structure is adapted to vision, it is at present only a matter of conjecture, or perhaps I should say probable inference, in what respect the functions of the rods and cones differ from each other.

It is to be remarked that the layer of nerve fibres is the first thing the light meets with after passing across the vitreous humour. Hence the light must go right across the layer of nerve fibres (as well as certain other intermediate layers) before it can reach the bacillary layer. Hence the mere passage of the light across these nerves, nerves of vision though they be, produces no sensation of vision, nor indeed sensation of any kind. If it did, light might be perceived as such, but there could be no distinct vision. For it appears to be a rule that the stimulation of a given nerve produces a given sensation, no matter to what part of the nerve the stimulus be applied. Hence if one of these visual nerve fibres were capable of being stimulated directly by light, the visual sensation corresponding to it ought to be produced (mixed it may be with other sensations) when the point from

which the light came occupied not merely one but a whole series of positions, becoming a line when projected on the visual sphere, those namely whose images on the retina lay on the various points of the nerve fibre in question.

The same conclusion, namely, that the nerves of vision are not directly stimulated by light, follows from the well-known phenomenon of the blind spot of the retina. The nerve fibres belonging to the layer mentioned above unite in the optic nerve, which runs into the brain, being led out of the eye through a hole in the sclerotic, not in the axis of the eye, but to one side towards the nose. Now images of points in the field of view must be formed on this spot as well as elsewhere on the retina, but the common experiments in relation to the *punctum cæcum* show, that no visual sensation is produced by the light which falls here. This oblique position of the optic nerve is accordingly a matter of the utmost importance to us. We might perhaps have expected at first sight that the fibres would have been arranged symmetrically with respect to the axis, and have been led out into an optic nerve at the centre of the back of the eye-ball. Had this been so, we should have been blind to the point of the field at which we looked, and for a little way round it. As it is, the centre of the field, which is the place where vision is

most distinct, is seen with both eyes, and though there is a small patch to the right of the centre of the field for which the right eye is blind, it is taken up by the left eye; and similarly as regards the blind spot of the left eye.

The well-known experiment of Purkinje's figures affords a further proof that the nerves are not stimulated by the light which crosses them. In this experiment the forms of the blood-vessels of the retina are seen in the field of view, in consequence of the partial interception of the light which falls upon them. This shows that they must be situated in front of, and not far from, the percipient organ, so that their shadows may fall on it as on a screen; and their motion, when the candle used in the experiment is moved, shows on the other hand that they do not actually touch it. Now it is found on dissection that the blood-vessels run among the nerve fibres, and therefore the percipient organ must be situated a little further back, that is, a little outwards, reckoning from the centre of the eye-ball. Such is the situation of the layer of rods and cones.

The office of the nerve fibres appears to be simply one of conveyance, notice of a change of condition of the recipient organ at the nerve end being conveyed in some way by the nerve to the brain, and then in some manner which seems likely always to remain a

mystery, giving rise to the sensation. Perhaps the subject may be made clearer by a rough analogy drawn from common life. Take the sending of a telegram. The image painted on the retina is analogous to the message handed in to the telegraph clerk, the percipient organ to the instrument manipulated by the clerk, the nerve to the line wire, the sensation to the delivery of the message at the other end.

The general surface of the outer ends of the rods, or in the central part of the retina where there are no rods, of the cones, is in contact with a layer of cells containing a black pigment; cells which also extend some way inwards so as to come between the rods, and still further inwards when stimulated by light, so as in that case to reach to the cones, with which they are in contact independently of the stimulus of light, in that part of the retina where there are no rods to keep them off. The office of the black pigment is generally supposed to be to absorb stray light, like the lamp black with which the optician coats the inside of the tubes of his telescopes. Since however light must be more or less absorbed in order to produce a change in the percipient organ, and there is no substance in the neighbourhood, at least in the case of the human eye, capable of exercising so intense an absorption as the black pigment, it has been doubted whether its office may not be

more direct. But that it is not at any rate essential to vision is shown by the fact that it is wanting in albinos, who nevertheless are able to see.

What the particular part of the rods or cones may be at which the change takes place from light to some effect produced by light which gives rise to the sensation of vision, and what the nature of that effect may be, are questions to which we are not at present able to give determinate answers. It has I believe been suggested that the outer segments are the seat of the change, and that the change is of a photochemical nature; that is to say, that under the influence of light certain chemical decompositions take place, and that the new products thus formed act as a stimulus on the nerves.

It is noteworthy that in most vertebrates the outer limbs of the rods are suffused with a purple colouring matter, which has been called visual purple, which has the property of being turned yellow and then bleached under the influence of light, while in the dark the purple colouring matter is regenerated provided the eye is sufficiently fresh, and the rods are in contact with the choroidal epithelium. The visual purple possesses accordingly some of the properties which we should expect on the supposition that vision is produced by a photochemical action: but its changes are not sufficiently prompt to allow us to suppose that

it is through its means that vision is obtained, besides which it has not been found in the cones, but only in the rods, and some animals are destitute of rods, nor are there rods, but only cones, in the part of the human retina which is in the immediate neighbourhood of the axis, though objects in the corresponding part of the field of view are not only seen but seen with special distinctness.

There are several arguments which may be urged in support of the photochemical view, which has much to commend it. At the same time I confess that it seems to me not altogether exempt from difficulty. The sensation remains as long as the eyes are kept open and light supplied, with no apparent change beyond a somewhat greater sensitiveness of the organ on the first admission of light after it has been kept for some time in the dark; and yet when the light is cut off, as by removal or failure of the source or by closing the eyelids, the sensation seems immediately to stop. I am not speaking, you will understand, of the phenomenon of so-called after-images, but of the ordinary sensation of vision. It is true that the cessation of the sensation is not instantaneous, for the steadiness of the impression produced by a rapidly fluctuating light shows that the sensation lasts for a finite though short time, which has been estimated at the tenth of a second, and it is much enfeebled in

a considerably shorter time. It may be remarked in passing that the duration depends upon the colour; being distinctly longer for blue than for red light. We cannot of course say for certain that this demonstrates a brief finite duration in the change of state produced in the percipient organ, though that appears to be the most probable explanation, for it might depend on the time that the influence on the end of a nerve takes to travel along it, though the time seems much too long for that if we may judge by the time an influence takes to travel along nerves of touch; or again it might depend on the sensation of impressions in the sensorium.

Now, supposing the duration of the impression to depend on the duration of a change of state in the percipient organ, it seems to me that there is one physical phenomenon in which a change of state is produced by the action of light, the behaviour of which in respect of duration is strikingly analogous to what we require in the percipient organ. I allude to phosphorescence of brief duration, of such duration that it might be called indifferently phosphorescence or fluorescence. Take for example uranium glass. The glass is yellow by transmitted light, from the absorption of the most refrangible rays. But if we regard it in such a direction as to look across the rays, as soon as the light is let on we see a green

colour, which is due to phosphorescence. This remains as long as the light continues to fall upon the glass; and when the incident light is cut off, the green phosphorescence seems immediately to cease. It lasts however an appreciable time, the thousandth of a second or so, as may be shown by Becquerel's phosphoroscope and by other means.

Now the change of state in the glass, the existence of which is made known to us by the phosphorescence, is, so far as the relation between the time of action of the exciting causes and the duration of the effect goes, precisely such a change as we require in the percipient organ. This tempts us to enquire whether possibly the change in the latter case may not be of a similar nature to the change in the former. Now, what takes place in the glass is doubtless this: the incident ethereal vibrations throw the ultimate molecules of the uranium compound into a state of internal agitation, and they in turn become centres of disturbance to the ether, and so give out light. When the incident light is cut off the molecular agitation does not at once cease, but rapidly dies away, partly by communication to the ether, but mainly, as I have long thought, by communication of an agitation to continually widening groups of neighbouring molecules of the glass, which form vibrating systems of greater extent and increased period of

vibration. When the light falls continuously on the glass the molecular agitation tends to be renewed as fast as it tends to die away, and a permanent condition is maintained. Now, if a similar kind of molecular agitation is excited in the ultimate molecules of the substance or substances of which the percipient organ is composed, and if being excited it is able to affect the nerves, we have just such an apparatus as we require.

I throw out this conjecture for the consideration of physiologists, of whom I have no pretensions to be one myself, and merely as a conceivable alternative to the photochemical theory which it seems worth while to bear in mind, even though the latter should appear the more probable. I would just observe that the presence or absence of fluorescence in the percipient organ would not by itself alone go far either to confirm or to refute the suggested explanation. For fluorescence is so common in organic substances that it might very well be present without having anything to do with vision. On the other hand, molecular agitations similar in their nature to those in uranium glass might very well be present, and yet not give rise to any visible light, as their period or periods might be such as to belong to rays beyond the red.

When a person uses a telescope for viewing objects at different distances, and some of them not far off, he

is obliged to re-focus his instrument in passing from one object to another, as otherwise he would not see them all distinctly. Now the eye acts as a telescope, and if it remained invariable in form we could not see quite distinctly objects at very different distances. The focal length of the eye being but small, an object at a moderate distance would be as good as at an infinite distance so far as sharpness of vision is concerned, and therefore a person who could see distant objects distinctly, would also see distinctly objects at a moderate distance without any change in the eye. But when we come to nearer objects, as in reading, the vision would be indistinct, unless assisted by spectacles, if there was not some change in the eye itself answering to the re-focussing of a telescope. But we are provided with such an adjustment (at least during youth and middle age, for the power is diminished or lost in old age), by the exercise of which we can see distinctly at very different distances. What the changes are which constitute the adjustment has given rise to some conjectures, but it is now ascertained to consist essentially in an alteration of curvature of the anterior surface of the crystalline lens. The lens being somewhat more refractive than the aqueous humour, an increased curvature of the anterior surface shortens the focus, which is what is required for distinct vision of a nearer

object. The adjustment ordinarily accompanies, apparently automatically, the voluntary act of making the axes of the two eyes converge on a nearer or more distant object, though some persons appear to have the power of altering the adjustment at will independently of an alteration in the convergence of the axes.

Hitherto I have spoken only of the perception of light as such. But the objects which we see are not presented to us simply in light and shade as in a photograph; we see them with a great variety of colours, which contributes very much to our enjoyment, and helps us in the ordinary concerns of life by the means of discrimination which it affords. The means whereby the difference of sensation produced by lights of different composition as regards refrangibility is brought about, are far from being understood; we can only feel our way towards a partial explanation.

It has long been recognised that there appears to be a triplicity of some kind about the various colours which we are able to perceive, as if they were referable to a mixture of three primary colours, though some confusion was formerly made in the subject from regarding the colour produced by a mixture of coloured pigments as the same thing with a mixture of the colours which the pigments separately exhibit, which in fact is a totally different thing. Now, assuming the

existence of say three primary colours, the difference between them might be objective or subjective; that is to say, there might be three kinds of light, usually mixed together in any light presented to us, each affecting us with the same sensation as to colour, different from that with which the two others affect us; or else we might have as it were three senses in relation to light, such that if one were alone affected we should have a particular sensation as to colour, which would remain the same though other circumstances, such for example as the refrangibility of the light, might change. On the former supposition, an element of a pure spectrum would contain three kinds of light though not separable by refraction, the proportions of which would change from one part of the spectrum to another; on the latter, the light of the element would be homogeneous, but would be capable of exciting our colour senses simultaneously, but in proportions differing according to the place of the element in the spectrum.

Of these two suppositions, the second, which is that of Dr Young, is by far the simplest. For even if there were a triplicity in the object, we should still require a triplicity in our organization in order that the objective difference might be subjectively perceived as a difference; whereas, if a triplicity in the organization be admitted, we have no need to assume a triplicity in

the object, of which we have no experimental evidence. It is true indeed that Brewster thought that he had succeeded by the use of absorbing media in modifying the tint of a given part of a pure spectrum. But the phenomena on which Brewster relied have since been shown to be due to illusions of contrast. There are cases indeed in which the apparent tint of a given part of the spectrum changes somewhat with the intensity of the light, independently altogether of contrast. But this affords no proof that light homogeneous as to refrangibility is nevertheless heterogeneous as to colour.

The experiments of Helmholtz and Maxwell appear to show, that the supposition of the existence of three primary colour sensations suffices to account by their union for the various shades of colour which we perceive. In particular Maxwell has shown by his colour top, and more recently by mixing colours of the spectrum, that if we take three standard colours X, Y, Z, any colour C may be expressed by the formula

$$C = aX + bY + cZ,$$

where a, b, c are numerical coefficients which may be positive or negative; $=$ means matches in colour and intensity; $+$ means superposed on, and $-$, in case any of the coefficients should be negative, means that the term must be transferred to the other side of the equation. If the standard colours are well chosen the

coefficients a, b, c in most cases will be positive. The best colours to choose for standards appear to be red, green and blue.

Microscopic examination of the retinas of the eyes of mammals has not hitherto revealed the existence of a difference of structure in the different elements which would naturally suggest a triplicity of function. We have it is true rods and cones, but in the central parts of the human retina there are only cones, which appear alike; and yet in the centre of the field our appreciation of colour is specially good. But it is remarkable that in the retinas of birds and reptiles the inner limbs of the cones are furnished with highly coloured globules, which the light has to pass through on its way to the outer limbs. The actual colours appear to vary somewhat from one kind of animal to another, but to be generally yellow, ruby, colourless, and a few green. The globules of one colour may be more numerous than those of another, but the globules of the different colours appear to be very equally mixed among one another. So remarkable a structure can hardly be imagined to be destitute of function, and it has naturally been supposed to be connected with the perception of colour by these animals. The cones in the same species of animal may be classified according to the colour of their globules, and it seems not unreasonable to suppose that the sensation excited

by the stimulation of the fibres coming from cones of one class may differ from that produced by the stimulation of those from cones of another class, and that this difference may correspond to a difference of the colour perceived.

It can hardly be doubted that when light produces vision it is absorbed in doing so. Now the coloured globules absorb the rays of part of the spectrum and let pass those of another part. The ruby globules, for example, let through the red rays and absorb those that are more refrangible. If we suppose the globule and its immediate neighbourhood to be the percipient organ, the stimulating rays will be those above the red, the assemblage of which would excite in us the sensation of green. But if, as seems more probable, the organ in which light first produces those changes that result in vision be the outer limbs of the cones, the globule would stand as a porter at the gate, letting through none but the red rays, which on this latter supposition would be the stimulating rays.

The retinas of mammals however are not provided with these coloured globules, which shows that at any rate colour vision may be obtained without them. They are nevertheless so far confirmatory of the theory of three primary sensations of colour as that they show the existence, in certain races of

animals, of different classes of cones, those of each class being on the whole very equally mixed among those of the other classes; and it seems not at all unlikely that the nerves which end in the cones of these different classes respectively may on stimulation give rise to sensations of different kinds respectively. In the human eye the cones on being traced inwards lead to a sort of fine thread, which at its base divides into excessively fine fibres. It has been conjectured that of these different fibres may on stimulation give rise to different colour sensations, so that each cone would as a rule furnish fibres of each class: and a plausible conjecture might be offered to account for the proportion between the stimulation of the fibres of different classes being different with exciting lights of different refrangibility. On this supposition, the parts of objects which were only distinguishable from one another by colour would be more sharply defined with man than with those classes of animals where different colour sensations belong to different cones, if indeed such be the case with those animals which are provided with coloured globules. But I fear I have been indulging too much in what is only speculative.

I have hitherto considered almost exclusively the action of a single eye with reference to vision. But though we have two eyes, each of which gives us

perfect vision of an object, which for the present I will treat as a point, when we use both eyes together we do not ordinarily see the object as two but one. It is perfectly easy, however, to see it as two. We have only to squint, or if we prefer it, to hold a small object in a line with the first, but at a different distance, and direct our eyes to it, and instantly the first object is seen double. In the first case, the axes of the two eyes were directed to the object, and its images fell on the centres of the two retinas, in which case as we have seen the sensation is that of a single point in the field of view. In the second case the axes are directed to the second object, which I will suppose to be nearer than the first, and will treat as a point, and it accordingly is now seen single in the centre of the field. But as the first object lies to the right of the axis of the right eye, its image falls to the left of the centre, and it is accordingly seen by the right eye to the right of the second object. Similarly it is seen by the left eye to the left of the object. In a similar way it appears that if the eyes be directed to the first object, the second is seen double; but now, as seen by the right eye, it appears to the left of the object seen single, and similarly as regards the other eye.

Suppose now, that while the eyes are directed to the near object the further one is moved to the right.

Both its images will appear to move to the right, and presently they will both appear to the right of the second object but at very unequal distances from it, the one previously to the right, which is the one seen by the right eye, being the more distant. Suppose now, the first object brought nearer to the eyes than the second and placed in a line with it. Then, of its two images, it is now the one to the left that is seen by the right eye. Hence, if the first object be moved to the right, till both its images are seen to the right of that of the second, the one seen by the right eye is now the *less* distant from it. Now suppose the first object held always to the right of the second, so that both its images appear to the right of the second, and let it be moved from a distance from the eyes decidedly greater than to a distance decidedly less than that of the second object. Then the distance of the image seen by the right eye from that of the second object will at first be the longer, and at last the smaller of the two distances. Hence, as the first object is moved continuously from the first to the second position, for some intermediate position the two distances will be the same, and the first object will appear single though it is to the second that the eyes are directed. Hence, not only do the centres of the two retinas correspond, in the sense that when the images of a point fall on them the point is seen single,

but other pairs of points possess the same property. In fact, to each point of one retina corresponds a point in the other in the sense above indicated.

According to this definition it would be a matter of experiment to determine the relations between the positions of corresponding points in the two eyes; and as we cannot at all accurately judge of the coincidence or non-coincidence of position of two points when they are well out of the axis, near which only we see with full distinctness, the experimental determination could not be very accurately made. When however we look straight forward at one point of a distant object the rest of the object appears single. Hence, the two points on which fall the images of any one point of the object are corresponding points, at least within the limit of errors of observation. This condition is accordingly usually taken as the *definition* of correspondence; though I confess it seems to me that the natural definition is that derived from singleness of vision; and the fact that under the condition named, the two images of the same point in the object fall on corresponding points of the two retinas, is to be taken as a proposition established by experiment.

There can be no doubt that the stimulation of a given point of the retina of either eye by the image of a point falling upon it, produces a given sensation

no matter how the eye-ball be turned in its socket. Suppose then, we have single vision of an object, say a distant object, using both our eyes. Then the pairs of foci from the various points of the object fall on pairs of corresponding points on the two retinas. If now, while one eye-ball remained fixed, the other rotated round any axis, immediately the pairs of points on which pairs of foci fell would become non-corresponding, and we should have double vision. Hence, there must be a determinate adjustment between the angular positions of the two eyes in order that vision may be single.

Now the most general angular displacement of a body, and accordingly of either eye-ball, may be given by three rotations about determinate axes, suppose vertical, horizontal running right and left, and horizontal running forwards. Now it is remarkable that the eye-ball is provided with six muscles, one straight pair in a horizontal plane the contraction of one or other of which turns the ball in one direction or the contrary round a vertical axis; one straight pair in a vertical plane, which similarly turn it round a horizontal axis running right and left, and a third pair of oblique muscles, which turn it round its own axis. By means of these the requisite relative adjustment of the eye-balls can be and is effected.

The horizontal muscles are in constant use for not

merely turning but turning unequally the two eyeballs, as we have occasion to make the axes converge on objects at various distances, from a few inches up to a large distance for which the axes may be deemed parallel. We have no occasion to make them diverge, but there is no difficulty in doing so up to a few degrees of divergence, by holding before one eye a slender prism with its edge vertical and its thick side next the nose, and viewing a distant object till it appears single.

Unlike the horizontal, the vertical straight muscles are not called upon in ordinary life to act differently for the two eyes. But that they are capable of doing so may be shown by looking at an object and holding a slender prism with its edge horizontal before one eye. The object is at first seen double, one image being above the other, but if the separation be not too great they can be readily united after a little. The experiment, as Maxwell showed me, can be made with still simpler apparatus, nothing but a pair of common spectacles being required. Viewing an object through the spectacles, turn them gradually and not too much round an axis passing through the ridge and perpendicular to the planes of the glasses. One can easily maintain single vision, and if the spectacles be now suddenly removed the vision is found to be double at first, one image being over the

other. This shows that the vertical muscles afford sensible play for adjustment.

Lastly, the use of the oblique muscles in adjustment may be shown by a simple experiment which I have not seen mentioned. Take two slips of glass varnished at the back to stop the second reflection, and holding them one under each eye view by reflection at a high angle of incidence the images say of the string of a blind as it hangs down and is seen against the sky. Adjust the glasses to make the images blend into one in the most comfortable manner, and then turn one of the glasses slightly round an axis pointing forwards. The two images are seen to cut at a small angle, but on continuing to gaze at them they presently blend into one.

How the unity of sensation between corresponding points is brought about, is more than we can tell. The optic nerves after entering the brain unite in the optic commissure, from whence run nerves to the right and left sides of the brain. The course of the fibres in the commissure, and the extent to which the fibres from the right eye cross over to the left side of the brain or keep to their own side, are not I believe as yet made out for certain, so difficult is the histological investigation of these fine and complex structures. What the object of this mode of crossing may be, and whether it has anything to do with singleness

of vision, is not at present known. But whatever be the mode in which the unity of sensation is brought about, there appears to be no doubt that as a matter of fact the elements of the retinas of the two eyes do correspond in pairs, and that the six muscles of the eye-balls supply the mechanism required for adjustment to single vision.

LECTURE IV.

Original object of the Burnett Trust—Possibility at first sight of contemplating the motions of the heavenly bodies as having gone on as they are from a past eternity—Such a view negatived by their physical condition—This scientific conclusion greatly strengthened when we consider living things—Scientific investigation is adverse to the hypothesis of the spontaneous origination of life—Our inability to explain by mere natural causes the vast variety of forms of life, and their changes in geological time—Difficulties of the Darwinian theory if regarded as a solution of the problem—Evidences of design afforded by an examination of the structure of living things—Marvellous adaptation of the eye to its uses—Self-existence, beyond which we cannot go, not attributable to the visible universe—The evidence of design leads to the contemplation of a designing mind, of whom self-existence has been affirmed—Further evidence derived from the study of mind—Inadequacy of the human mind to take in together the ideas of personality and exemption from limitations of time and space—The character of God revealed to us through the Son.

IN the order of the Home Secretary by which a new direction was given to the Burnett Foundation, it

is prescribed that the Trustees shall instruct the lecturer to have regard, in treating of the special subject prescribed, to the illustration afforded by it to the theme proposed by the testator. Hitherto I have touched only incidentally on this topic; but I think that I shall not be acting otherwise than in accordance with the wishes of the Trustees by devoting the present lecture, the last I shall deliver as Burnett Lecturer, to this special subject.

Let us in imagination so separate ourselves from ourselves as to use our intellect, while conceiving ourselves unconscious of our own existence and of all that that involves; and under this restriction let us contemplate the heavenly bodies, more especially those of the Solar System, from the point of view of an astronomer who observes and records the places of these bodies from time to time, and reduces their motions, at first complicated and to a considerable extent lawless, to order under the guidance of the law of universal gravitation, but who avoids, as foreign to his subject, any speculation as to the physical condition of the bodies other than that they consist of matter obeying the law of gravitation. Such a person ascertains that the motions of those bodies take place in accordance with the results of his calculations. He finds that he is able to determine years beforehand what the places of those bodies shall be, and when

the time comes they are found in their predicted places; or, if there be minute discrepancies beyond the limits of casual errors of observation, he finds that by revising his calculations, which are excessively complicated, and can only be carried out approximately, though there is no limit to the accuracy to which the calculations may with patience be carried, these minute discrepancies gradually disappear; or if there be still a slight outstanding error the presumption is that it only needs a yet still more refined and laborious calculation to account for it. There is no intimation of any error in the principles on which the calculation is founded; and it is proved mathematically that according to those principles the motions are periodic, and capable of indefinite continuation. It is true that the orbits in which the planets move are subject to very slow variations, but these secular changes are themselves found to be periodic, though the periods are enormous compared with those of the bodies in their orbits. And just as the motions of the bodies are capable (in accordance with the principles to which we have hitherto confined our attention) of indefinite continuance in time to come, so they may be determined from our formulæ for any past time however remote. For anything that appears so far, the heavenly bodies may be self-existent, in the sense that they are capable of continuing as they are for

ever, and may have existed as they are from a past eternity.

But when from considering merely the motions of the heavenly bodies we examine into their physical condition, even though we still continue to ignore the existence of plants and animals, ourselves included, the case is very different. The geological examination of the superficial portions of our earth, to which alone we can get access, points to a succession of changes extending indeed over vast periods of time, but progressive rather than periodic in their nature. There are indications of volcanic agency in remote times on an immense scale; and various considerations, derived in part from a study of the figure of the earth independently of geological speculations, lead to the probable inference that the earth was originally in a molten state. The telescopic appearance of the surface of the moon points to the former existence of powerful volcanic forces now extinct. In the case of the moon, and to a great extent in the case of the earth, we are without evidence of the present existence of a residue of primitive heat; but the sun is always pouring forth heat into space by radiation. This, as was observed in a former lecture, is analogous to a continual expenditure of capital, and is not therefore a process calculated to last for ever, or which can have been going on from a past eternity. Similar conclusions

as regards the earth might be deduced from a consideration of the effects of tidal friction, which must tell in the long run; but the consequences of this are less easily followed, and what has already been mentioned is sufficient for our purpose.

The upshot is that even if we leave out of account all organization, whether of plants or animals, we fail to find in the material system of nature that which we can rest on as self-existent and uncaused. The earth saith it is not in me, and the sun saith it is not in me.

When from the contemplation of mere dead matter we pass on to the study of the various forms of life, vegetable and animal, the previous negative conclusion at which we had arrived is greatly strengthened. We can conceive of suns and systems as formed, under the operation of laws open to our investigation, from masses of matter previously in the condition of fiery nebulæ. I do not say that they were actually so formed, but only that there is nothing opposed to what we know of the laws of nature in the supposition that they were; and modern researches have even lent some additional degree of probability to the supposition that such was their origin.

But as regards living things the case is different. As I have already remarked, a physical examination of the condition of our earth gives us reason to think

that the earth was at one time in a molten state, a state accordingly in which no living thing, be it plant or animal, such as we know, could exist. How then did it become furnished with the abundance of life in various forms that we see around us at the present day, and that the examination of fossil remains shows to have existed in ages far back in geological time?

Two questions are here involved; first, that of the origin of life on earth in any form, and secondly that of the origin of the various forms in which we find it, forms that have changed from one geological age to another.

As to the first, some of the ancients imagined that maggots were spontaneously generated in decaying flesh, and that carcases bred bees. It needed however no great study of natural history to dispel such notions as these, and as to the larger animals there could never be any question that they were the offspring of others like them. We have however animals varying in size from the whale or the elephant to creatures so minute that it requires a microscope to show them at all. It stands to reason that the germs of these last, if germs there be, would be of such minuteness that even the microscope could hardly if at all reveal them. Such organisms are constantly found in connexion with putrefying sub-

stances, even though the closest search fails to reveal the germs, if germs there be, from which they spring; and the question has been seriously debated, even in recent times, whether creatures of such a kind may not sometimes at least arise spontaneously from dead matter. The experimental investigation of this question, as will readily be understood, is beset with great difficulties, so hard is it to effectually exclude germs so excessively minute as must be those of these microscopic organisms, if indeed they do come from germs. The result of the experiments which have been made in this subject by the most careful workers is such that most persons are I think now agreed that the evidence of experiment is very decidedly against the supposition that even these minute creatures can be generated spontaneously.

Here then, in the origin of life, we have a problem which science fails to give any account of. It differs from that of the origin of the celestial bodies in this respect, that it does not send us back to a time so remote that we are unable to contemplate any orderly system of nature anterior to it. On the contrary, if, as science seems to indicate, our earth were at one time in an incandescent state, very considerable progress must have been made towards the condition in which we find it at present before it would be adapted for a habitation to even the simplest forms of

living things that we see around us or find remains of in a fossil condition. We have then evidence in the commencement of life on earth of the operation in time—and not merely at some indefinitely remote time which we please to contemplate as that of the origin of things—of a cause which for anything that we can see, or that appears probable, lies altogether outside the ken of science.

One conjecture has indeed been thrown out which if it were seriously entertained would invalidate the evidence of the operation in time of some ultra-scientific cause, that is derived from contrasting a former azoic condition of the earth with its present condition and that which it has had during geological ages now long since passed, but subsequent to the azoic period. In his presidential address at the meeting of the British Association in Edinburgh in 1871, Sir William Thomson suggested as a conceivable mode of conveying some form of life to the earth that possibly a meteorite containing the germs of some low organism might have fallen on the earth, and that these germs might have given rise to a living progeny. Of course such a supposition if adopted would leave untouched the problem of the origin of life; it would merely invalidate the argument for the origination of life on our earth within geological time.

I need not however dwell on the many formidable difficulties which stand in the way of any such hypothesis; for example the intense superficial incandescence produced in a meteorite by its rapid passage through the air, for I do not conceive that the hypothesis was ever meant to be adopted. I can say for my own part that as I listened to the address I did not suppose that the distinguished speaker threw out the hypothesis as one to be really entertained; he was only, as I understood him, illustrating in a graphic manner the nature of a *vera causa* as distinguished from would-be explanations which refer the origin of life to something of the nature of an occult quality.

But the existence of life upon earth is not the only problem which presented itself to us; there is a vast variety of forms of life upon earth; can we give any scientific account of the origin of these forms?

I need hardly remind you that one famous theory has been put forth to account for the origin of species, which is due to a most distinguished naturalist who has not long since departed from among us. In this theory there are postulated, first, the existence of life to start with, secondly the continuation of life by reproduction, thirdly, the general resemblance of offspring to parents, whether of plants or animals, combined however with minor variations which we are

obliged to regard as accidental, fourthly, a tendency towards the hereditary transmission of any special peculiarity, any deviation, that is, from the average normal type.

These postulates being assumed, it is held that in the struggle for existence consequent on the limitation of space and food supply, individuals with such variations from the average type as are advantageous in the struggle will have a better chance than those that are otherwise constituted, and are likely to live longer and produce a more numerous progeny; and as the progeny have on the whole a tendency to inherit the peculiarities of their parents, there is thus a one-sided tendency towards an improvement in the race, improvement in the sense of its becoming better fitted for its environment. This change may be conceived to be continued till a maximum is reached, when any slight deviation from the normal type would be disadvantageous rather than otherwise, and thus the form of maximum advantage would tend to be perpetuated; and just as, to use a mathematical illustration, a function of several independent variables may admit of a variety of maxima, so on this hypothesis we may conceive of a variety of forms of life, each possessing the quality of a maximum of adaptation to its environment, these forms answering to different species.

The amount of transmutation of form which can be obtained in actual experiment is small indeed compared with the interval which separates remote forms of life; nor is it by a process analogous to the survival of the fittest that the more remarkable even of these transmutations have experimentally been brought about. But it is held that the time at our disposal is merely infinitesimal compared with the ages which such changes required in the natural way. In fossil remains however we have records, fragmentary it is true, and more or less discontinuous, of forms of life which have existed far back in geological ages. We might have expected to find here evidence of a continuous change from one of what we call species into another, if such there were. While however there is a general progress in the character of forms of life, when looked at in the large scale, the links which might have been expected are for the most part wanting. This the advocates of continuous transmutation account for by supposing that the records of intervening forms have been swept away and destroyed by the convulsions which from time to time wrought great changes in the earth's crust in the past. Some on the other hand are of opinion that an examination of the records of the past is unfavourable to the hypothesis of continuous transmutation. Thus Sir William Dawson expresses the opinion that an

examination of fossil remains points rather to outbursts of fresh forms of life from time to time in past ages; forms which seem to tend rather to deteriorate than to improve.*

It would ill become me to express an opinion of my own in branches of science which like geology are out of the track of my own studies. Suffice it to observe that if as regards the first origin of life on earth science is powerless to account for it, and we must have recourse to some ultra-scientific cause, there is nothing unphilosophical in the supposition that this ultra-scientific cause may have acted subsequently also.

Hitherto I have spoken of living things and the various forms of living things merely in relation to their existence. They cannot it is true be thought of wholly without regard to their structure; but hitherto the structure has been kept out of view, being reserved for separate consideration. Let us now turn our attention to this point.

The most cursory examination of living things, especially the higher animals, cannot fail to impress us with the adaptation of their structure to their mode of life and their wants. This is perhaps most strongly felt when some one organ is taken, and studied in considerable detail. Let us take then the

* The chain of life in geological time. Published by the Religious Tract Society.

eye, suppose the human eye, the structure of which in relation to its functions was to some extent dwelt upon in my last lecture. What a wonderfully refined organ it is that we are here presented with. While the other parts of the body are opaque or merely translucent, we have here a ball an inch or so in diameter as clear as crystal. The form of the cornea, the form of the crystalline lens, are such as an optician would choose for the refraction of pencils of light that were to be brought to foci on the spherical surface of the back of the eye itself, and that, even including refinements which are neglected when as is usual in elementary books we confine our attention in the first instance to the so-called geometrical foci to which extremely slender pencils would be brought, but the neglect of which would entail imperfections of vision not indeed fatal to the use of the eye, but interfering with its full efficiency. Then we have that delicate self-acting screen, the iris, which regulates within wide limits the quantity of light that is allowed to fall upon the retina, and so guards that network from permanent injury or temporary dulling of sensibility which might be occasioned by an excess of light, while at the same time allowing a marvellous sensitiveness to feeble light. Then we have that close set carpet of ends of nerve fibres with their rods and cones, forming

an exquisite mosaic which it requires a microscope to reveal, bodies the operation of which is not understood, but which appear to be adapted to convey to the sensorium individual sensations corresponding respectively to individual points in the field of view. Then we have a remarkable arrangement of muscles adapted to permit of the adjustment necessary for single vision, by causing the images of a point looked at to fall on corresponding points in the retinas of the two eyes: though why it is that the stimulation of the nerve fibres leading from corresponding points should produce the same sensation, is more than we can explain.

When we contemplate the mosaic of the human retina, with its elements regularly arranged, and set at distances of only one or two ten-thousandths of an inch apart, and think of these almost countless elements as destined to convey the impressions of the almost countless points which we can distinguish as separate in the field of view; still more when we think of the correspondence of the two eyes and of all that that involves—that the mosaics should be of the same pattern and very approximately at least of the same size; that their elements should be brought into correspondence two and two in a perfectly methodical manner, those elements in the two eyes corresponding which agree in distance from

the centre and angle of position; when we consider the number and fineness of the fibres leading from the elements and into the brain—when I say we contemplate all this, it seems difficult to understand how we can fail to be impressed with the evidence of Design thus imparted to us.

I am aware that some see in all this only the operation of the law of the survival of the fittest, through which it is supposed, if we grant the postulates which the theory requires, the whole structure, complex and elaborate as it is, arose from some excessively simple beginning, from some lowly organism in which nothing of the kind existed, merely through the consequences of casual variations from the original type. Even if this were granted, it would not follow that no evidence of design was left; but can we grant it as even a probable hypothesis, for no one I suppose would hold it to be proved? The process supposed in the theory may be one real feature in a very complex whole; namely in the existence of the various forms of living things, both vegetable and animal, that we behold; but that we want nothing more to account for the existence of structures so exquisite, so admirably adapted to their functions, is to my mind incredible. I cannot help regarding them as evidences of design operating in some far more direct manner, I know not what;

and such I think would be the conclusion of most persons.

But design is altogether unmeaning without a designing mind. The study then of the phenomena of nature leads us to the contemplation of a Being from whom proceeded the orderly arrangement of natural things that we behold. Thus we are led to place in a Being this attribute of self-existence which we failed to find in the races of living creatures, or even in the majestic march of the planetary bodies, though ages may elapse before any deviation can be observed from the periodic motions which they execute in accordance with the law of gravitation. And in the present connexion it is noteworthy that it is precisely this attribute of self-existence that God himself chose for his own designation. When Moses was commissioned to go to his countrymen the Israelites and announce their coming deliverance from Egyptian bondage, and enquired by what name he was to call Him who sent him, the reply is, "Thus shalt thou say unto the children of Israel, I AM hath sent me unto you."

At the beginning of this lecture I asked you in imagination to ignore your own existence, while applying your minds to the contemplation of inanimate nature. From thence we passed on to the consideration of races of living things, which led

us on to their structure, and in particular to the structure of a very marvellous organ of our own bodies. So far however we have merely contemplated our bodies as we might have done the planets, as something external to ourselves. But let us now pass on to consider our minds—the human mind. Here we can no longer contemplate something external to us, for we touch our inmost selves. There is some very intimate connexion between thinking, as we know it in ourselves, and the condition of the brain. So close is the connexion, that some have supposed that thinking is a mere function of the material organism, conditioned by nothing more than the motions of the molecules of which that organism consists. But surely this is going far beyond a legitimate inference from the observed facts. The body of a living animal is obedient to the laws of motion, the law of gravitation, and similar laws of the kind which belong to dead matter. But that does not prove that life is nothing more than a process depending on such laws. So if thinking be accompanied, as we know it in ourselves to be accompanied, by a state of activity of the material organism of which the body consists, that does not prove that thinking is nothing more than an action of the material organism. We have seen that life can only proceed from the living; may it

not be in a similar manner that mind can only proceed from that which has mind? See what the contrary supposition leads us to. Here is man, in a geological sense a creature but of yesterday, utterly incapable of accounting for his own existence by any play of mere natural forces, and yet ignoring the existence of any mind higher than his own mind, though ready enough to admit the existence of unintelligent law, and that, without limitations of time or space.

As then the indications of design in the material construction of our own bodies lead us to the contemplation of mind in that from whence they originated, so the consideration of mind as it exists in ourselves points in the same direction. We are led therefore to attribute personality to that in which alone we can rest as the first cause of all.

But I would not for a moment be understood as if it were through science only, or even through science mainly, that we are led to a conclusion so important. Man's intellect does not form the sum total of his mental powers. He is endowed with feelings and aspirations, and has a sense of right and wrong too universal to be attributed to the result of education, though of course capable of cultivation. This points to a power above him; and it may be doubted whether a nation ever

existed so rude and barbarous as to be destitute of the idea of a power higher than man. Thus considerations derived from totally different sources converge towards a common conclusion.

But when we speak of the First Cause as personal, it must be remembered that human language fails us in attempting to describe the infinite. When we think of a law of nature, the limitations of space and time do not enter into the conception. Take for example the law of gravitation. We speak of it as universal gravitation; we think of it as operative in the past ages to the contemplation of which we are led in our geological speculations as well as at the present day; we regard it as holding together the components of the most distant double star as well as maintaining in their orbits the planets of our own system; but we do not think of gravitation as a power endowed with mind. On the other hand when we speak of a person we can hardly avoid thinking of our own personality, and of the limitations of time and space to which we are subject. We find it hard to put the two ideas together—that of personality, and that of exemption from the limitations of time and space. Yet each mode of conception helps to supplement that which is lacking in the other. If we shut our eyes to the grandeur of nature, and do not attempt through the things that are

made to acquire higher conceptions of the eternal power and Godhead of the Maker, our conceptions of the Divine Being are apt to become too anthropomorphic. If on the other hand we confine our attention to the study of nature in all its immensity, our conceptions of its Author are in danger of merging in a sort of pantheistic abstraction, in which the idea of personality is lost.

Are we then left to lose ourselves in an ocean of immensity, and driven to the conclusion that God is unknowable? Nay, as Christians we believe that the character of God has been revealed to us as it never had been before through that Divine Being who took our nature upon him and dwelt among us full of grace and truth. The greatness of the universe displays to us something of the greatness of its Author; but when we study the character of the Son, who is the image of the invisible God, we learn as never had been learnt before the lesson that God is Love.

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